3.

Obſervations

ON THE

HISTORY AND CAUSE

OF

ASTHMA;

AND

A Review of

"A Practical Enquiry on Diſordered Reſpiration;"

IN A LETTER TO

ROBERT BREE, M.D.

The Author of that Work.

BY GEORGE LIPSCOMB,

SURGEON, AT BIRMINGHAM.

&c. &c.

Birmingham,

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1800.

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To the Public.

A VARIETY of malicious rumours, induſtriouſly circulated, and encouraged from motives of envy, or of perſonal enmity, having unfortunately ſo far ſucceeded in prejudicing the minds of the public, as to occaſion the Author an unuſual degree of leiſure; he thought that he could not employ himſelf in a manner more conducive to the benefit of his fellow-creatures, or more becoming his *profeſſional* reputation, than by endeavouring to elucidate the hiſtory of a very prevalent and diſtreſſing diſeaſe, which has been hitherto but ill explained, and very unſucceſsfully treated.

At the ſame time he embraces the opportunity, which the preſent publication affords him, of aſſuring thoſe liberal friends, who ſtill honour him with their confidence and patronage, that neither time nor accident can diminiſh the grateful ſenſe which he

entertains of their kindneſs; and while he deeply regrets the inſurmountable obſtacles which have prevented him from publicly refuting calumnies, which, in the multifarious ſhapes they have aſſumed, have *always* been ſo guarded by artifice, or modelled by intrigue, as to elude any *direct blow*, even if his fortune had afforded him the means of ſeeking that redreſs to which the juſtice of his cauſe entitles him :—he reſts, with confidence and aſſurance, on the continuance of their protection, whoſe friendſhip for him has either been founded on habits of intimacy and long eſtabliſhed acquaintance, or has generouſly ariſen out of a regard for juſtice, and an hatred of oppreſſion.

He earneſtly hopes that Time (the univerſal Phyſician) the rectitude of his conduct, and his profeſſional induſtry, will gradually obliterate any unfavourable impreſſions which may have been made on thoſe, who, being ſtrangers to him, have had no opportunity of inveſtigating the cauſes of the

perſecutions

perſecutions which he has undeſervedly ſuffered.

The loſs of the pretended attachment of ſome who, actuated by motives of intereſt or of policy, were once ſolicitous of being reckoned in the number of his friends; and who, whenever a *proof* of their ſincerity ſhall be demanded, will deſert their new aſſociates with as much puſillanimity as they have deſerted him; and with as much alacrity as they have fled from the performance of ſolemn promiſes and reiterated aſſeverations; is not to be conſidered as a real misfortune: and ſuch he *condeſcends* to mention, only that he may remind them, as he has already obſerved in another publication* that "*popu-*
" *lar applauſe is lighter than a feather or a*
" *bubble, and leſs ſubſtantial than a dream*;" and that although the friendſhip of the GOOD and the WISE will ever be the object of his

* A Journey into Cornwall, &c. interſperſed with remarks, moral, hiſtorical, literary and political, 8vo. Rivingtons, St. Paul's Church Yard, London, 1799.

higheſt

highest ambition; yet he trusts he shall constantly support that manly independence of spirit, which will enable him to sustain, without a single emotion of regret, the loss of any intimacy with those, who, however superior to him by birth or station, have no claim to the epithet of *wise* or *virtuous*.

And if there yet remain some malignant spirits who experience a delight in the invention or propagation of calumny and detraction: if the itching ears of envy and credulity still feel a gratification in listening to the clamours of scandal, or the whispers of falsehood:—to these he desires to say, that although they may *rob* him of "a *good* report," they cannot deprive him of a *good* conscience: nor shall all the efforts of envy, of malignity, or of persecution, make him relinquish the pleasing hope, of still contributing to *advance* and to *improve* the practice of the profession, *in* which he is engaged, *for* which he was educated, and *to* which he is in some measure naturalized, by

its

its having devolved on him, through a respectable line of ancestry.

The Author thinks it would be impertinent to trouble the public, with any explanation of the circumstances which have induced him to select Dr. BREE's "Practical "Enquiry *on* Disordered Respiration," for the particular subject of his animadversions: but as a literary controversy, on a professional topic, between a Surgeon and a Physician *resident in the same Town*, will, perhaps, be supposed to have arisen from personal pique, envy, or jealousy; it may be proper for him to obviate the possibility of any such misapprehension, by informing his readers, that Dr. BREE is equally unknown to him as a gentleman and a physician.

Birmingham, March 10, 1800.

ERRATA.

Page 20, line 10, for ? insert .
Note, line ult. for humanä read humanâ.
27, line 8, and 19, and in other places, for distention read distension
34, Note, line 7, for "Willis," read Willis
52, Note, line 7 for allisque, read alüsque
55, Note, line ult. after sensation, insert ?
68, line 16, for with, read and
79, Note, line 6, after nerves, insert ;
81, Note, line ult. for Expements, read Experiments.

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OBSERVATIONS

ON THE

HISTORY AND CAUSE

OF

ASTHMA,

&c. &c.

SIR,

THE perusal of your "Practical Enquiry *on* Disordered Respiration," having induced me to consider the Nature and Cause of Asthma with some attention, I take the liberty of addressing to you a few observations on this subject.

I do not think it necessary to preface my remarks with an apology for their freedom, because every Author who introduces to the world either an history of new facts, or an illustration of principles already established, is amenable to society for the consequences of his opinions, and obnoxious to the criticism of those who doubt the truth of his reasoning, or dissent from his conclusions.

Although your "Enquiry" had made its appearance more than two years before it happened

to fall into my hands; neither the length of time which has elapſed ſince its publication, nor the difficulty of combating ſome opinions which appear to me objectionable, without expoſing myſelf to the imputation of ſatyrical malevolence or captious iraſcibility, ſhall therefore deter me from what I conceive to be a ſerious duty to the public:—from ſtating, without reſerve or equivocation, how far the doctrines contained in your "Practical "Enquiry" have a tendency to produce any uſeful improvement in the method of treating Convulſive Aſthma; and how far your reaſonings on that Diſeaſe have elucidated the hiſtory of its nature and cauſe.

I read your publication, Sir, with that degree of attention which the diſcuſſion of a ſubject ſo highly important ſeemed to require, and will confeſs that, when I reflected on the want of preciſion among Authors, which you have been pleaſed to adduce in your preface, in vindication of a new arrangement of the cauſes of Aſthma; when I reflected on the important aſſiſtance which has been derived to the ſtudy of Medicine from innumerable recent improvements and diſcoveries in the ſiſter ſciences; it afforded me pleaſure to find, that a gentleman of learning and induſtry had choſen this Diſeaſe for the particular object of his ſtudies and inveſtigation: and, when I underſtood that the author's clinical obſervations had been aided by a "perſonal "acquaintance with the diſeaſe," I looked forward with

with eagerneſs and confidence, for the attainment of ſome uſeful additions to the general ſtock of medical knowledge.

It is not, however, to complain of my diſappointment, that I take the liberty of addreſſing you; but that I may, with all fairneſs and all candour, inveſtigate the degree of credit due to your theory, and the degree of attention due to thoſe principles of reaſoning, on which you have grounded the practice you recommend.

They who read, only to know the opinions of others, forget one of the moſt obvious and important uſes of human intellect;—they forget that this invaluable gift was beſtowed upon them, that they might be enabled to weigh every ſentiment, and to appreciate every remark:—they ſhould ſtudy as well as pronounce, ſhould be cautious in the admiſſion of new theories, and examine the validity of every argument by the rules of juſt criticiſm; ſhould yield nothing to the emptineſs of ſounding titles, or the dogmas of pompous erudition; but minutely regarding thoſe ſubſtantial and indefeaſible arguments, which are only founded on obſervation and experience, ſhould reject every innovation, and diſcountenance, every affected improvement, which will not ſtand the teſt of actual examination.

Every Author has an undoubted right to make uſe of what terms he pleaſes; but whenever he

 introduces

introduces an epithet not generally underſtood, or not uſually applied in the ſame ſignification as that in which he uſes it, it is incumbent upon him to define the ſpecific meaning which he intends to convey.

Phyſic, in itſelf an abſtruſe ſcience, has become almoſt unintelligible, from the immenſe quantity of ill-defined terms which have been introduced, by thoſe who pretended to illuſtrate the ſtudy of it. To underſtand the language of ancient writers, replete with ambiguity, and ſtuffed with a farrago of quaint and indeterminate epithets, is now a taſk ſo arduous, that few ſtudents have ſufficient patience, to combat this formidable obſtacle to the improvement of Science:—but although this circumſtance, by compelling men, who poſſeſs more of genius than of induſtry, *to think for themſelves*, has been in a certain degree, inſtrumental in the production of thoſe valuable improvements in the different branches of Medicine, which have diſtinguiſhed the preſent age; it is to be lamented, that the cuſtom of embarraſſing the reader with technical terms, never well defined, and conſequently ſeldom perfectly underſtood, has been extended even to our own times, and, I am ſorry to ſay, has reached your "Practical Enquiry *on* Diſordered Reſpiration."

But I do not forget that I am to combat your *opinions*, rather than the *words* in which they are conveyed: that I am not to attempt a review of the

the whole of your work, to marſhal its inaccuracies, or to expoſe the peculiar ambiguity of, what a little faſtidiouſneſs might denominate, its inflated periods. I haſten, therefore, to fulfil the taſk which I have undertaken, without further preface or introduction.

Your firſt ſection is devoted to an anatomical ſketch of the Lungs, and other organs of Reſpiration; and, a mere general view of theſe parts being all which is required, to elucidate the hiſtory of Aſthma, I ſhall not cavil at the want of a more accurate deſcription; nor, as I know the difficulty of adapting ſpecific terms, capable of illuſtrating many of the appearances which are obſervable in the animal machine, am I captious about the "blood-veſſels ſpread * and interwoven like the "meſhes of a net," or "the perfect † net-work" formed by the lymphatics: although neither of them may be very correctly explained by theſe alluſions. But, I muſt confeſs, that the inference which you have deduced from the ſlender ſize of the nerves which ſupply the Lungs, appears ſcarcely reconcilable to general obſervation.

You remark, that "the ‡ pulmonary nerves "being ſo ſmall, the lungs have little ſenſation." Does the acuteneſs of ſenſation, then, depend on

* Dr. Bree's Practical Enquiry on Diſordered Reſpiration, p. 4.
† Ibid.
‡ Ibid. and 171.

the

the ſize of the nerve expoſed to the influence of ſtimuli? Certainly not: for a much more perfect and correct idea is produced in the mind, from the infinitely minute terminations of the nerves of the ſkin, than ariſes from the application of any ſtimulus to a large ſurface, or a large proportion of the greater nerves.

Let us attend a little to experience. The fimbriated extremities of the olfactory nerves, diſtributed to the membrane which lines the noſtrils, are ſurely not deficient in ſenſibility; for they may be ſtimulated by a portion of matter ſo ſmall, that we have no means of affixing to it any determinate idea of quantity: although their origin is not ſo large but that they are frequently broken in diſſections, by raiſing up the brain, in order to demonſtrate their exit from it. And to return to the lungs;—it will not be denied, that the pain felt in peripneumonic inflammation is at leaſt equal in degree, to the pain which accompanies inflammation of any other membrane of the body.

Whence happens it, Sir, that if the lungs have "little ſenſation" they ſhould be ſo exceſſively liable to be irritated by every atom of extraneous matter received into them? Are the nerves you *allow* them, diſtributed only to the "**intermediate* "intervals" which you ſpeak of, between the lobules? Or, how is your doctrine of the inſenſibility of this

* Practical Enquiry, p. 2.

viſcus,

viſcus, reconcilable to the irritability of the delicate membrane which lines the Trachea and its branches, even in their minuteſt ramifications?

I perfectly agree with you, that "the lungs* "have no inherent power by which they can attract "air;" and will acknowledge that when I firſt read the remark, I was a little puzzled to know who had ſuſpected the contrary: till I recollected that *Ambroſe Parey*, more than two hundred years ago, among other wiſe ſuggeſtions, had ſtarted an idea, that the lungs had "*a diſpoſition to imbibe* pus, which in "pleuriſies and other purulent abſceſſes," ſaid he, "is poured † into the cavity of the thorax."

I come in the next place to conſider an obſervation more ſingular than, and I apprehend equally indefenſible with that reſpecting the inſenſibility of the lungs.

"Phthiſis ‡ and Aſthma," you remark, "comprehend the features of every ſerious indiſpoſition "which can generally attack the lungs, and they illuſtrate mutually the character of each. It will be "found, that in their regular, ſimple, and uncompli-"cated forms, the remedies of one are the exciting "cauſes of the other, and the cauſes of the one are

* Practical Enquiry p. 5.

† Amb. Parey, Opera, lib. iv.

‡ Practical Enquiry, p. 11.

"recipro-

"reciprocally the remedies of the other." This paradoxical aſſertion I cannot think of holding up to the admiration of my readers, with any other view, than that it may poſſibly contribute to the improvement of Science; or, to uſe your own words, *to the gain of humanity*, by a diſplay of error.

In refutation of the former part of the ſentence, I ſhall only beg leave to remind you, that the diſeaſe called Hydro-thorax, appears juſtly entitled, by its oppreſſive ſymptoms, and fatal effect, to be ranked with Aſthma and Phthiſis, as, at leaſt, "*a* "*ſerious indiſpoſition*" affecting the lungs.

If the poſition which you have advanced in the latter, be admitted, it will indeed afford a moſt cogent reaſon for never attempting the cure of Aſthma; ſince you have diſcovered, that ſuch an apparent benefit would neceſſarily be ſucceeded by a diſeaſe to which, you acknowledge, that "me-"dical* treatment is oppoſed, without hope, and "without ſucceſs:" which by the way, ſeems to render it extremely problematical, whether you are acquainted with the exciting cauſes of Aſthma; for, if the "remedies of Phthiſis" (according to your preceding remark) be the exciting cauſes of Aſthma, and if Phthiſis be an incurable diſeaſe; *What are its remedies?*

* Practical Enquiry, p. 11.

Again;

Again;—in another place you have ſaid, that Aſthma is "excited* by irritation," and you *allow* it to proceed from "various remote cauſes."—May I aſk then, which of the remedies employed for the removal of the *irritation*, can you poſſibly dread, as the exciting cauſe of Phthiſis? and which of thoſe remote cauſes of the irritation, you have alluded to, do you conceive likely to remedy Phthiſis? Vinegar is one of the remedies for the paroxyſm, on the uſe of which, you have laid a conſiderable ſtreſs: I have, therefore, ſelected it as the ground of another queſtion.—Do you conſider it capable of exciting Phthiſis? One more queſtion, and I have done. You have particularized, "ſevere ſtudy" as one of the remote cauſes of a prediſpoſition to Aſthma.—Do you think it would at all conduce to the cure of Phthiſis?

On the merits of your definition of Aſthma, I would wiſh to ſay a few words.

It is avowedly taken from thoſe of *Willis* and *Hoffman*, and partaking of the nature of both, is liable to the objections which have been made to each of them. For myſelf, I hold all noſological diſquiſitions as of but ſmall importance, in the hiſtory, and conſequently in the ſtudy of diſeaſes. A definition can only be a good one, when it conveys a clear

* Practical Enquiry, p. 11.

and correct idea of a diſeaſe, and exhibits the outline of its character, in ſuch a light, as to diſtinguiſh it from every other diſeaſe.

In order to define a diſeaſe accurately, it muſt be perfectly underſtood: but a diſeaſe may be perfectly underſtood, ſo far as the limited powers of the human underſtanding can be ſaid to attain *perfect* knowledge, and yet it may be almoſt impoſſible to give an exact definition of it.

Much labour and ſtudy being required for that purpoſe, it is to be feared, that while Authors have been racking their brains for comprehenſive terms, whereby they might define diſeaſes, a great portion of time and of induſtry has been waſted, which might have been more advantageouſly employed in the inveſtigation of the ſymptoms and progreſs of thoſe diſeaſes, and in the eſtabliſhment of opinions founded on clinical experience, which is the only true baſis of practical knowledge.

You have been pleaſed to quote ſome authorities, on the quantity* of atmoſpheric air received at each inſpiration; and have noticed the curious phenomenon of divers in the pearl fiſheries remaining under water, for a conſiderable length of time. I take leave to introduce a circumſtance which you ſeem to have overlooked, namely, that in perſons accuſtomed to diving from an early period of life, the *foramen ovale* remains open. The freedom of the circu-

* Practical Enquiry, p. 20, 21.

lation,

lation, therefore, in these amphibii, is not so much interrupted, as would otherwise happen: and as water is known to contain a much larger proportion of *oxygen*, than is mixed with atmospheric air, may it not be suspected, that a certain quantity of this necessary fluid is absorbed into the system during submersion?

I do not remember by whom the fact I have mentioned, was originally discovered; but it was well known in the last century, and frequently demonstrated by *M. Dionis*, a famous French Surgeon and Anatomist.

In Newfoundland-dogs also, the *foramen ovale* is found open. I have, myself, once seen it, in the adult subject, and from the circumstance of the individual being a native of the coast of Africa, and a sailor, I was induced to suppose that he also might have been a diver; but respecting this, I had no opportunity of obtaining authentic information.

You remark, that some "animals* pass whole "winters in sleep, and emerge from it in spring "with *increase of fat*." This does not, however, accord with general observation: for it is well known, that Bats are frequently discovered in their torpid state, but that they are invariably found *lean*, in a degree proportionate to the length of time

* Practical Enquiry, p. 21, 22.

 which

which has elapſed, from the commencement of their protracted night. The Marmot alſo, another of the animals whoſe exiſtence is obſcured by torpid inactivity, is expreſsly deſcribed, as having acquired a degree of plumpneſs at the approach of winter, but at the time of its reſtoration, (in the month of April) as being conſtantly* reduced to a ſtate of great emaciation: and even in appearance, almoſt a ſkeleton.

The quantity of oxygen required for the maintenance of the vital principle, varies greatly, in different animals; and perhaps, in the ſame proportion as the degrees of temperature of their reſpective bodies.

It is obvious, that a much ſmaller quantity of air is received into the lungs during ſleep; and, that whenever a perſon has ſlept in the ſame clothing commonly worn when awake, the ſenſation of cold is always perceived, as ſoon as the ſleep terminates. Thoſe animals which ſleep for ſeveral months together, have their natural heat conſiderably diminiſhed.

The Toad, which affords, perhaps, the moſt ſingular example, in the whole ſeries of animated nature, of long abſtinence from pure air; or rather, of life being kept up, by a very ſmall quantity of oxygen, may likewiſe be adduced in proof of what

* Martyn's Dict. of Nat. Hiſtory. Art. Marmot.

has

has been advanced. I have, however, ſometimes entertained an idea, that the laſt named animal may poſſibly be furniſhed with the means of throwing out from its body, *gas*, in a ſtate capable of recombination with other ſubſtances, in the very confined atmoſphere around it, (as, for example, in the cavity of a block of marble, little larger than its body) ſo as to form a quantity of oxygen ſufficient for its nouriſhment.

I am not informed whether this animal becomes emaciated or not, in conſequence of its confinement, above alluded to: but as all the reſt of this claſs of ſleepers are reduced in ſize, ſo it is probable that a century or two, may produce *ſome little effect* on the toad likewiſe.

I paſs over, in total ſilence, the numerous deſcriptions of morbid appearances on diſſection, which with ſo great induſtry, and even labour, you have collected from ancient writers; becauſe, in ſome caſes, it is confeſſed, and, in others, it is highly probable, that *no aſthmatic affection* prevailed.

It is an incontrovertible fact, that the lungs may be gradually compreſſed, to almoſt half their uſual ſize, ſo that the inferior parts of the lobes ſhall remain impervious to the air, inhaled by the larger branches of the trachea; and no Aſthma be produced: of which I have myſelf been an eye witneſs, in the caſe of *William Hunt*, of the pariſh of St. *Nicholas*, in *Warwick*, who died of hydrothorax;

thorax; the particulars of which may poſſibly, hereafter, be preſented to the public.

I ſhall not trouble you with any remarks on your diviſions of "convulſive Aſthma," in the ſecond part of your "Practical Enquiry;" nor on the "*Aſthma Exanthematicum*,* proceeding expreſsly "from the retroceſſion of an exanthematous acri-"mony," but leave it, to ſpeak for itſelf, *in its own words.*

But, I cannot omit the paſſage, in which you mention the increaſe of energy, as ſometimes proceeding to a degree, which in itſelf conſtitutes a diſeaſe. You adduce, by way of example, the effect of "acrid Bile" in the following words. "Acrid Bile† in the inteſtines occaſions extraor-"dinary periſtaltic motion, and if that irritating "material *is* not diſcharged from the bowels, by "the ordinary exerciſe of their powers," (I preſume you mean *ſuch extraordinary* periſtaltic motion) "an inverted motion will ariſe," &c.

A great deal has been ſaid about this *inverted motion*, and even of the inteſtinal canal being tied in knots. Whoever conſiders, for a moment, the form and connexion of theſe organs, will not, however, require any argument to convince him, of the impoſſibility of the latter effect being pro-

* Practical Enquiry, p. 75.
† Ibid. 161.

duced,

duced, unleſs the inteſtines could be previouſly ſeparated from their connecting bands, the meſentery and meſocolon. As well might the ruffle of a ſhirt be tied in knots, while it maintains its attachment to the wriſt-band.

In ſhort, it is a fallacious ſtatement, deduced from the ſenſations of the patient; and entirely unſupported by any real evidence whatever.

It is very true, that Introſuſceptio does take place; and probably, in conſequence of extraordinary excitement of the periſtaltic motion; but even this is far from proving that the motion has been *inverted.* It rather proves the contrary, at leaſt in ſeveral inſtances, which I have had an opportunity of examining, and where the recipient portion of the tube was invariably found to be the inferior continuation of that part which it enveloped. Now, it is well known, that no *probability* of inverted motion can be urged, from any difference of ſize, in the cavity of the jejunum or ileum, and that the tube of the colon, which is moſt frequently, the ſeat of the diſeaſe, is nearly of an equal diameter throughout its whole length. There is, in fact, but one proof which can be adduced of the occurrence of inverted action, to wit, the rejection of *ſcybala* through the ſtomach. This is ſo rare an occurrence, that a practitioner may enjoy very extenſive opportunities of obſervation, without meeting with it once, in his whole life. Being therefore ſo

uncommon

uncommon, it ought not to be advanced as a foundation for general reaſoning: beſides, it ſhould be remembered, that even in ſome of the few caſes of the diſeaſe which have been authenticated, the occurrence of this ſymptom was induced by a repetition of violently active emetics; which may be allowed capable of effecting an alteration in the ſyſtem, which the uninterrupted progreſs of a diſeaſe would never have produced. If, however, the periſtaltic motion were reverſed in caſes of introſuſception, we ſhould undoubtedly often meet with the inferior portion of the inteſtine intruded upwards.

And, as very great inflammation, in all caſes; and, even in moſt of them, a mortification precedes death; it frequently happens, that there is great difficulty in determining the preciſe ſpot at which the diſeaſe commenced: and the experience which I have had of this difficulty, has led me to ſuſpect, that the accounts delivered to us, of the appearances, on diſſection, have been ſometimes not very correct.

It is much to be regretted that the opportunities of anatomical reſearches are ſo often denied, to the commendable ſolicitations of the faculty:—were theſe advantages more general, every man of induſtry and talents, would be enabled to form an infinitely more correct idea of the nature of thoſe diſeaſes, which are connected with evident diſorganization,

nization, than he can ever obtain, by the moſt attentive peruſal of Bonetus or Morgagni.

The ancients, who undertook their diſſections frequently with a view to the illuſtration of falſe, and now, juſtly exploded theory, were extremely apt to have their minds groſsly biaſſed by ſome favourite hypotheſis; and we, who enjoy the clearer light of modern diſcoveries and improvements, may readily trace, in their writings, the unſucceſsful efforts which were employed, to prop up the viſionary ſuperſtructure, founded on their imagination, rather than on their judgment. Of this, I ſhall have occaſion hereafter to ſpeak more particularly.

The inflammation produced in ſome caſes of introſuſception, as I have before obſerved, is known to proceed even to the death of the part: and it frequently happens, that a perfect adheſion of the folds takes place, and that a maſs of granulations is formed, of a large ſize, with which the folds of the inteſtine are ſo conſolidated, that the preciſe origin of the diſeaſe cannot poſſibly be traced.

An inſtance of this kind occurred in my own practiſe, and was demonſtrated on diſſection, in the body of *Thomas Blackford*, of *Barford*, near *Warwick*. The diſeaſe in this particular caſe had been undoubtedly occaſioned by the injudicious and exceſſive uſe of cathartic medicines. A very large portion of the colon was protruded through the ſigmoid flex-

ure, where its duplicatures were united, and consolidated in a vast mass of granulations, which for some time previous to his death, formed a considerable tumour externally; and ultimately obliterated, entirely, the cavity of the intestine.

From a consideration of these circumstances, I can easily conceive that where the origin of the disease has been rendered obscure, and the mind of the examiner predisposed to the belief of inverted action, some cases may have been adduced in support of a favourite theory, which, a more unbiassed examination would have had a direct tendency to contradict.

You appear to be anxious, Sir, for the establishment of an implicit dependence on the "accu-"racy of observation," as well as the "fidelity" of ancient authors, whose works you are pleased to call "classical."

I request your attention to a few remarks, which have been suggested by observing the deference you every where pay to the sages of antiquity.

The early writers on physic, have filled their works with a great deal of nonsense about *black bile*, and its deleterious* consequences. Has any single

* Hippocrates maintained this opinion with so much vehemence, that, as his learned commentator has observed, "Hippocratis sententia "fuit omnes putridas febres a bile nasci."

dissection,

diſſection, from the creation of the world to the preſent day, tended to countenance even the ſhadow of ſuch a theory? and yet, do we find our anceſtors the more inclined to part with their favourite opinion?

This obſtinate adherence to erroneous doctrines is not, perhaps, to be urged in objection to the weight of their authority, as clinical hiſtorians; but, as that obſtinacy reſulted, in a great meaſure, from the inaccurate knowledge which they had, of the ſtructure and natural hiſtory of the body; I am myſelf ſo much a modern, that I will ſometimes venture to doubt the correctneſs of their ſtatements reſpecting the appearances after death. Nor, do I conceive, that I am unworthily detracting from the merit of the ancients, when I endeavour to place them in the ſituation, to which their induſtry and exertions have really entitled them; although in a niche ſomewhat lower than that to which they have been undeſervedly raiſed, by the devotion of their ſuperſtitious admirers.

What is it? but to offer the reſpect of rational and deliberate opinion, inſtead of blind and inconſiderate adulation. I regard the writings of the ancients, as objects of criticiſm, rather than of delight; and although I endeavour to ſtudy them without prejudice, I am cautious not to ſuffer the garb* which envelopes them, to awe me into veneration.

* The eye of philoſophy ſhould penetrate the covering which time has woven to conceal the errors of ancient writers; and, I hope,

It is impoſſible to know the true value of the taliſman, till the charm which guards it be diſſolved. The real form of the ſaint cannot be underſtood, till the ſhrine which encloſes him be examined: and ſhall he who reſpectfully approaches, to view with the eye of reaſon the long concealed treaſure, be accuſed of ſacrilege? as if with unhallowed hands, he had preſumed to violate the ſacred incloſure, or with profane impiety, to ſcatter the holy relics.

In the darker ages of phyſic, and before the knowledge† of anatomy was either generally, or in-

we may freely inveſtigate the credit due to their authority, without violating THE SPIRIT of the well known axiom, "de mortuis, nil "niſi bonum:" to which I moſt readily yield my aſſent, "for* though "(after the approbation of the good and wiſe) one cannot wiſh any "thing better than to be heartily abuſed by the envious and malignant "in this life, becauſe it is as certain a ſign of one's merit, as a dog's "barking at the moon is of her brightneſs; yet the vail which death "draws over us, is ſo ſacred, that the throwing dirt there, has been "eſteemed at all times, and by all people, a profanation." While we admit the truth of this remark, however, let me obſerve, that a precept of far greater importance in point of morality, is too often and too commonly diſregarded—"de viventibus, nil niſi verum."

* Warburton.

† Among a thouſand other abſurdities, it has been aſſerted, that the bile of Negroes is black inſtead of yellow; and that the colour of their ſkin is occaſioned by a depoſit of this fluid in the cellular membrane. See Doctor Barrere's "Diſſertation on the phyſical cauſe of "the colour of Negroes." Could this LEARNED phyſician have ever ſeen the diſſection of an African?

Again, in one of the books which has been attributed to Hippocrates himſelf;—I mean the treatiſe "de naturâ humanâ;" it is ſaid that

deed almoſt at all underſtood, the opinions of thoſe who wrote on diſeaſes can be but of little importance. Their theory was, at beſt, only ingenious conjecture: their practiſe only uncertain empiriciſm. The dawn of a more reſplendent æra, at length appeared; and perhaps it had been well for mankind, if the ſun which then aroſe, had burnt up the ancient ſpeculations of theoriſts altogether.

Unhappily, however, even among the moderns, we ſtill find phlegmatic writers, who continue to look up to theſe "*blind guides*" with all the ſuperſtitious ardour of antiquity, and ſtill *venerate* the dignity of Greek and Arabian phyſicians, when their patient induſtry in recording the ſymptoms of diſeaſes has only entitled them to our eſteem.

Hippocrates, whom you, Sir, as well as ſome other modern authors, have again and again dignified, with the preeminent title of the "FATHER "of Medicine" (as if he had created it) may loſe no inconſiderable ſhare of his honours, and *all* his divinity, without any degree of biographical injuſtice. *Galen*, *Sennertus*, and *Riverius**, ſcarcely de-

that four pairs of veins ariſe from the head, and are diſtributed through the body! And although Galen might poſſibly have been influenced by other motives than the love of truth, when he aſcribed that work to Hippocrates, it cannot be denied, that both theſe authors, whenever they have attempted to reaſon on the principles of Anatomy and Phyſiology, have perpetually fallen into abſurdities, quite as ridiculous as that hypotheſis.

* "Attamen in magno per me ſervantur honore;
"Pulveris et cariem plumatis tergo flabellis."

ſerve

ſerve to be mentioned, unleſs in order to prove, that the moſt ſplendid talents, the moſt induſtrious perſeverance, and the livelieſt imagination, unaided by the benefits of diſſections accurately performed, and a clear * and comprehenſive view of the natural hiſtory of the human body, are inſufficient to conſtitute any thing like a knowledge of the nature and cure of diſeaſes. It is not, however, my intention to diſſuade the ſtudent from *peruſing* the works of the ancients, for it is impoſſible that a man ſhould know what *can be done* in the profeſſion of phyſic, without being firſt informed, of what *has been done* already; and without ſuch previous knowledge, he cannot tell whether he is the author of any improvement or not. I only mean to warn him againſt that *implicit confidence* in the early writers, which tends to the permanent eſtabliſhment of error, and to the total ſuppreſſion of ingenuity.

* The ancients ſuppoſed that BILE and PHLEGM were two of the cauſes of inflammation of the lungs, and that its degree of danger depended on the TEMPERAMENT (as they called it) of a patient, being more or leſs analagous to the particular nature of thoſe cauſes! ſupporting their opinions by arguments worthy of ſuch abſurdities.

Thus, the doctrine maintained by Galen was "Peripneumoniam "pituitoſam minus periculi habere quam bilioſam. Pulmonem au- "tem temperies eſt calida, minemeque frigida. Quare pituitoſa pul- "monis phlegmone parti, non erit analoga; ergo et periculoſior. "Pulmones ſunt molles, non duri: ſunt rari, non denſi: ſunt laxi: "ſi habitum inſpexerimus, facile animadvertemus bilioſam inflam- "mationem minime eſſe analogam ſubſtantiæ pulmonum: attamen "temperiei pulmonum analoga eſt hæc inflammatio bilioſa. Quare "AVICENNA ex GALENO BENE dixit, eryſipelatodem phlegmo- "nem pulmonis periculoſiorem eſſe pituitoſa."

I have

I have now arrived at your obſervations on the theory of ſpaſm, maintained by the late Profeſſor Cullen.

After quoting Dr. Stark's deſcription of the appearance of the lungs on diſſection; you add, "Diſſection * then, has not elucidated the myſtery "of ſpaſmodic conſtriction of the bronchia, but it "will be replied, that the ſpaſm is relaxed before "death, and thus eludes the examination of ana-"tomy. It is, however, remarkable that this ſtate "of conſtriction has not appeared in ſome inſtances "after death, when an attendant diſeaſe of more "rapid and *fatal progreſs* than the Aſthma, may "have *finiſhed* a patient under the dominion of the "paroxyſm of the latter."

With all due deference, Sir, if one of the eſtabliſhed laws of animal death, be a relaxation of the muſcles of the body (and I call upon you to correct me, if it be *not*, by the hiſtory of a ſingle caſe, well authenticated, in which it has ever been departed from); it is evidently of no conſequence at all, whether the ſpaſmodic conſtriction has been occaſioned by Aſthma or by any other diſeaſe; nor to what particular cauſe the extinction of the vital ſpark be referred: for it will be *equally impoſſible* to *demonſtrate* the exiſtence of ſpaſm on diſſection.

What muſcles are found rigid in death? None. What fibres are diſcovered in a ſtate of contraction?

* Practical Enquiry, p. 175.

None.

None*. Spaſm, therefore, cannot be *demonſtrated* by any other diſſection but by the method which furniſhed the Greeks with all the little knowledge they had, of the ſtructure of the body, namely, by the unparalleled barbarity of diſſecting living men: a practiſe which the learned phyſicians among that *humane* and *enlightened* people, conſidered as much leſs painful and offenſive to the *manes*, than the diſſection of dead bodies.

But the want of ſuch demonſtration is no more a proof that ſpaſmodic conſtriction had not preceded the death of a patient, than the loſs of contractility in the ſkin, is a proof of its never having been furniſhed with the power of contracting during life;—a quality which I am perſuaded no man will venture to deny.

Hence, I contend that the inference which you have drawn, that "as† no appearance of ſpaſ-

* "There is a power conſtantly contracting the muſcular fibres "of a LIVING animal, to a ſhorter length than they would be con-"tracted by their elaſticity."

Dr. George Fordyce's "Second Diſſertation on Fever," page 75.

I ſhall have occaſion, frequently, to refer to this ingenious author, to whom I am indebted for many of the principles on which I have founded the arguments contained in theſe pages. In ſome inſtances, I have accidentally caught the Doctor's mode of expreſſion; in a few others I have adopted it, from choice. Far from being aſhamed, I am proud to acknowledge, that I have often BORROWED from my venerable Preceptor; but I think it neceſſary to make this confeſſion, that I may not be accuſed of having STOLEN from him.

† Practical Enquiry, p. 176.

"modic

" modic conſtriction of the bronchia has been dis-
" covered by diſſection, it is fair to conclude that it
" was not a part of the laſt condition of the lungs," does not neceſſarily follow from your premiſes, any more than if it ſhould be ſaid,—as no proof of ſingultus can be demonſtrated after death, it is impoſſible that a dying perſon could have ſuſtained hiccough.

Indeed, Sir, you ſeem to have entertained ſome doubt of the force of your own argument, when in another place you remark, that " though circum-
" ſtances* will not encourage the aſſumption of
" ſpaſm of the bronchia as a part of the paroxyſm
" of Aſthma, it is not intended to deny poſitively
" this condition."

But, while I only object to the weight of your arguments in oppoſition to the theory of ſpaſm; I requeſt that I may not be miſunderſtood, as contending in favour of Dr. Cullen's hypotheſis, of which I always thought it incumbent on him to have adduced more ſubſtantial proofs, than any which have yet appeared, in ſupport of it.

It is the bounden duty of every perſon who makes an *aſſertion*, to exhibit proof of its *truth*, inſtead of truſting its eſtabliſhment to the precarious reception of credulity, or to the lame and il-

* Practical Enquiry, p. 170, 171.

logical defence, ſo frequently ſet up, that *the contrary* cannot be proved.

In ſpeaking of the ſenſation of "ſtraitneſs," or, as it may be termed, obſtruction of the cheſt, in the paroxyſm; you obſerve—"The veſiculæ* are "occupied by lymph, the bronchia are ſtraitened "in their area by the ſame cauſe, in a more viſcid "ſtate, the diaphragm cannot deſcend as it uſed to "do, on account of the reſiſtance of the ſtomach, "filled and diſtended with flatus, and the trachea "is probably narrowed by the compreſſion of the "œſophagus, affected like the ſtomach."

It is extremely unfortunate, when a careleſs expreſſion happens to find its way into a book of ſcience, which has a tendency to miſlead the reader, on a point of great practical importance. I am far from attributing to you, Sir, a deſign to convey any other, than your moſt correct ideas of the ſtate of the organs you mention: but as erroneous conceptions are ſometimes produced in the minds of ſtudents, by looſe and popular terms, I have ſome apprehenſions of a ſimilar inconvenience from your remark of the ſtomach being "*filled and diſtended*;" and my fears are increaſed by a coincidence of expreſſion, in another part of your "Practical Enquiry."

* Practical Enquiry, p. 183, 184.

It

It is of the utmoſt conſequence that a fact, at preſent too much neglected, even by practitioners, ſhould be generally known, and attended to. I mean, that when even the ſmalleſt quantity of food has been received into the ſtomach, it is properly to be conſidered in a ſtate of plenitude, equally, whether that quantity be an * ounce or a quart; for although capable of great diſtention, it is always applied to its † contents.

But the expreſſions of which you have made uſe, appear primâ faciè, to ſupport the vulgar and erroneous idea that the ſtomach, like a bag, may contain a certain meaſure *without* being full; and although it cannot be ſuppoſed that you *intended* to be *ſo* underſtood, it unfortunately appears that you have, a ſecond time, countenanced the ſame notion, by ſpeaking of the ſtomach being "full ‡ or empty", in avowed contradiſtinction to exceſſive diſtention, on the one hand, and to perfect (that is, *real)* emptineſs on the other.

As to the *probability* of the Trachea § being narrowed by the preſſure of the œſophagus, when diſtended with flatus, which I perceive is hinted at,

* Fordyce's Treatiſe on the Digeſtion of Food, p. 13.

† Dr. Browne Langriſh's Croonean Lectures on Muſcular Motion, p. 46.

‡ Practical Enquiry, p. 251.

§ "Flatulence may force the ſides of the œſophagus againſt the "trachea, compreſſing the cavity of that pipe into a ſmaller area."
Practical Enquiry, p. 266.

in

in different authors, it ſeems to reſt on a very precarious foundation: for whoever will take the pains of making the experiment, will find it *impoſſible* to compreſs the hard cartilaginous rings of the larger branches of the Trachea, by the force of any diſtention, of which the œſophagus is capable;—not even if it be filled with a weighty fluid.

The wheezing, and the ſenſation of obſtruction may, however, be ſatisfactorily explained, without reſorting to ſuch an hypotheſis: and indeed you have yourſelf accounted for it, in a different manner,—by the "ſecretion * of mucus", and its accumulation in the bronchia.

Whatever degree of heſitation I may have felt, in reviewing your obſervations relative to the ſtomach; I have no excuſe to offer for criticizing upon the vague application of another term, whoſe importance in medicine ſhould prevent its being trifled with, or applied incorrectly.

* Practical Enquiry, p. 309. It is to be remarked, that this SUPPOSED diſtention of the œſophagus is ſaid to be "productive of wheezing where there is NO OBSTRUCTING MUCUS in the air paſſages "ſufficient to account for that ſymptom" (Practical Enquiry, p. 266) and that you have afterwards obſerved, "Wheezing is NOT a ſymptom, unleſs the long continuance of the fit ſhould induce a ſecretion "of MUCUS SUFFICIENT TO STRAITEN the bronchial paſſages;" to which you (I think UNFORTUNATELY) add, "I prefer PROBABLE CONJECTURE to deranging the induction eſtabliſhed in a "great majority of caſes, by aſſigning to SIMILAR effects, cauſes "DISSIMILAR." Practical Enquiry, p. 310.

You

You ſay, " Fever * is the ſyſtem of morbid " actions, which in Catarrh as well as Phthiſis is " adopted by nature to relieve the body of thoſe " affections."

Inſtead of conſidering it in this light, it has obtained, in my mind, a much higher rank in the claſſification of diſeaſes.

Fever is a diſeaſe, of which all the learning of the Greeks, all the induſtry of the Arabians, and all the boaſted wiſdom of the Egyptians, were inſufficient to afford the Practitioners of thoſe nations any accurate conception. That this term ſhould, therefore, have been applied incorrectly, or even at random in all ages, and in all nations, is by no means wonderful, when we reflect on its original ſignification having been derived from the idea of heat; and recollect in how vague and diffuſive a ſenſe that idea has been, and is entertained.

But, whoever regards with attention, the uſual operations of nature, or, in more correct language, the wiſdom of the Almighty in the organization of animal bodies, will immediately be ſenſible, how very erroneous have been the conceptions formed, reſpecting a diſeaſe whoſe † prevalence at all times,

* Practical Enquiry, p. 191.

† " It may therefore be ſuppoſed that the hiſtory of fever ſhould " have been very perfect by this time, ſince it is one of the moſt frequent " diſeaſes, and has been in all ages, and in all countries, more eſpeci- " ally

and in every country upon earth, has rendered it one of the moſt important in the hiſtory of medicine, and, at the ſame time, has afforded a conſtant opportunity, of its real nature being perfectly and completely underſtood.

That, at the termination of the eighteenth century, however, and aided by all the modern aſſiſtances, not merely of the improvements in Chemiſtry and Anatomy (which have contributed but little to perfect our knowledge of the nature of Fever) but by the important experience of clinical Practitioners, who have illuſtrated the hiſtory of the diſeaſe, by indefatigable reſearches, and the moſt patient induſtry:—that with all theſe advantages we ſhould ſtill, blindly and implicitly follow the erroneous doctrines of ancient theoriſts, or even adopt their modes of expreſſion, betrays a degree of profeſſional infatuation, which I hold to be utterly unworthy of this enlightened and philoſophic age: for we may truly apply to the errors and prejudices of *our* early writers, what has been ſaid, with great force, on a different occaſion—" Multa dicuntur " juxta *opinionem* illius temporis, et non juxta quod " *rei veritas* continebat."

Nothing has been more conducive to the eſtabliſhment of erroneous opinions reſpecting fever,

" ally as it is likewiſe one of the moſt fatal; and as it ſo occupies the " whole ſyſtem as to abſorb, during its continuance, all the faculties, " both of the body and the mind, in a greater or leſs degree."

Fordyce on Simple Fever.

than

than the idea ſo obſtinately adhered to, that it was a natural effort to throw out ſome *noxious matter* from the body; an idea which has deſcended from Hippocrates through innumerable authors, and which really ſeems to pervade your "ſyſtem of "morbid actions adopted by nature to relieve the "body."

I am aware that this has been the prevailing opinion of many *modern*, as well as ancient practitioners; and that it has been maintained with the moſt zealous obſtinacy,—in ſpite of abſolute want of proof, and in oppoſition to every day's experience. The whimſies of Paracelſus, and the dreams of Van-Helmont were not more abſurd. But although there has never been produced the ſhadow of proof, what the nature of this noxious matter, or turbulent ſpirit, might be; the diſeaſe, properly called Fever, has been found to ariſe in thouſands of inſtances, to go through its whole courſe, to be complete in all its ſtages, and at length to terminate, —ſometimes in health, and in other caſes, in the deſtruction of the body: wherefore, Fever is a diſtinct diſeaſe, and NOT *an effort of nature to carry off* matter whoſe retention would be prejudicial; and if Fever be a diſtinct diſeaſe, and not an effort of the ſyſtem to overcome a ſubtile ſpirit, or to concoct * groſſer matter, (which has never been

* Cheyne's Engliſh Malady, p. 201. See alſo Sydenham; and ALL the Ancients.

proved

proved to exist) it necessarily follows, that in those cases, where a disease, arising from the presence of sensible and demonstrable matter of any kind (as for example, of mucus in Catarrh) produces a particular state of the body, which is to carry off the original disease, that particular state, so, and for such cause produced, is NOT *the disease properly termed* FEVER.

Whenever, therefore, the term Fever be employed in a signification so widely different from its *correct* meaning, it ought to be accompanied by a proper elucidation, that the mischiefs which might result from the conveyance of an erroneous idea may be prevented; and that the reader may know in what sense the expression is to be understood*.

You have mentioned Sir John Floyer's error†, in supposing that *fever* occurs during Asthma; but,

* "Nothing, indeed, has disgraced physic more, than the duplicity "with which terms are adopted, and causes of diseases described, which "were never authorised by fact." Lettsom's Medical Memoirs, p. 3.

† "There is sometimes in Asthma a remarkable sensation PERVADES the body, accompanying the anxiety of the breast, and distinguished by an OBSCURE perception of Heat, and A WANT of cool "air. This perception of heat may have induced Floyer TO TALK "OF FEVER IN this disease." Practical Enquiry, p. 215.

The former part of the sentence is indeed A LITTLE OBSCURE; but it may be fair to apply to it your remark on Willis, "the LANGUAGE is perhaps MORE in fault than the conception:" and this may also be extended to another assertion, viz. that "in Asthma, the "system is INVIGORATED with TOO SMALL a proportion of oxygen." Practical Enquiry, p. 241.

I must

I muſt take the liberty of obſerving, that you do not appear to reject it, on what I conceive to be the proper ground:—for you afterwards remark, that "an attention * to the pulſe gives no counte-"nance to the obſervation;" becauſe "the quick-"neſs never amounts to one hundred beats in a "minute."

But ſurely, Sir, it is not your intention to aſſign the degree of celerity of the pulſe, as the criterion of the preſence of Fever! Were this the caſe, every man who rides a hunting, on a moderately warm day, might be ſaid to be in a fever. Every perſon, the paſſions † of whoſe mind were ſuddenly and vehemently excited, might be called feveriſh: rage and fear, love and jealouſy, might then be denominated fever: as well as an infinite catalogue of diſeaſes, which are not at all allied to it, in their nature or eſſence:—which neither accord with the ſymptoms of that diſeaſe, now properly termed fever, nor even with that which was conſidered as ſuch, in remoter ages, by the phyſicians of Greece and Arabia.

* Practical Enquiry, p. 215.

† Fordyce.

In the preface to the ſecond volume of Zoonomia, Dr. Darwin has thought proper to employ the word FEVER, as deſcriptive of a "quick pulſe which continues for ſome hours." I have before obſerved, that every author has a right to make uſe of his own terms; but NO AUTHORITY, excepting that of juſt reaſoning, ſhall compel me to accede to the propriety of their application. It ſhould be remembered, that quickneſs of the pulſe is not NECESSARILY preſent, to conſtitute the diſeaſe, properly called Fever.

In the ninth ſection of your "Enquiry," I meet with the following paſſage: "In Aſthma*, an *exceſs* "of blood, in the pulmonary veſſels, may very pro-"bably precede the exhalation of the finer part into "the veſiculæ and bronchia; this plethora ariſing "from the relaxed texture of the coats of the veſſels," and concluding with "blood letting is *never*, to *my* "†knowledge, uſeful, but frequently injurious."

When "an *exceſs* of blood" prevails, it appears ſomewhat extraordinary that blood-letting ſhould not be uſeful, by leſſening the degree of diſtention, which aſſiſts in ſtreightening the bronchial veſſels, and concurs with the effuſed ſerum to diminiſh the ſum of their cavities, at the ſame time, that it tends to deſtroy the contractile power of the blood veſſels themſelves.

It is not worth while to combat the idea of "*Plethora, ariſing from the relaxed texture of the* "*coats of the veſſels*," which is an aſſertion not to be

* Practical Enquiry, p. 192.

† Practical doctrines are, undoubtedly, recommended with conſiderable effect, by the authority of PERSONAL obſervation: but how unfortunate is it, that the experience of one author is ſo frequently contradicted by that of others! Not to mention, perhaps, hundreds of inſtances in which LIVING Practitioners have ſeen the beneficial effects of BLOOD-LETTING in the paroxyſm of this diſeaſe,—on looking into WILLIS, I find, that in one of THE TWO CASES with which he has illuſtrated his remarks "de Aſthmate,"—he expreſſly mentions the patient, (a nobleman, if that confers any weight on the hiſtory) being RECOVERED FROM THE JAWS OF DEATH by phlebotomy.

defended

defended by any argument deduced from reaſon or phyſiology: becauſe it has not yet been proved that the coats of the veſſels *are relaxed*, unleſs in thoſe caſes of Aſthma which ariſe in, or are ſucceeded by, an anaſarcous habit. This ſubject I am not, however, at preſent, to diſcuſs.

But, Sir, when you admit that Plethora does exiſt; when you allow that "there * are certainly "ſtriking circumſtances of analogy between Aſthma "and Apoplexy;" when *you agree* with Dr. Cullen, that an accumulation of blood in the veſſels of the brain, and an effuſion of ſerum, occaſioning compreſſion of that organ, are the proximate cauſes of apoplexy; and have grounded on that opinion, your remark, that "theſe † ſtates applied to the veſſels "of the lungs will be found to induce Aſthma;" you muſt permit me to expreſs my aſtoniſhment, that, in oppoſition to *ſuch doctrine*, you ſhould diſcountenance phlebotomy. But you have gone further, and remarked, that "there is *reaſon to appre-* "*hend*, an injudicious uſe of the lancet in pulmo- "nary complaints, has *brought on* Aſthma in ſome "inſtances‡."

As I differ from you reſpecting the cauſe of Aſthma, it will not be ſurpriſing that I ſhould not

* Practical Enquiry, p. 196.

† Ibid.

‡ Practical Enquiry, p. 296.

 concur

concur with you, on the above mentioned point: but, to enter into an argument about it, until I underſtand *the reaſon* of your apprehenſion, would be like fighting a ſhadow.

It may be thought captious to diſpute about an author's *belief*, when it is not productive of any ſuggeſtions likely to influence his readers; I will not, therefore, attempt to ſhake your *belief* of the *pneumonic inflammation* attendant on *intermittents*, and will only remark, that ſome men are extremely *prodigal of their belief*, and can ſcarcely with-hold it from "every wind of doctrine," or every *rumour of abſurdity*:—but there are others of a contrary diſpoſition, who will hardly believe the beſt eſtabliſhed facts, ſupported by the moſt reſpectable evidence. Freedom of belief, as well as of opinion, is happily ſtill the poſſeſſion of Engliſhmen; and is under the dominion of conſcience alone.

In your enumeration of the ſymptoms which take place in a paroxyſm of Aſthma, an "*itching* "*of the ſkin*" is mentioned, and attributed to "the "cauſes which more peculiarly occaſion the anxiety* "and ſtraitneſs of the cheſt,"—to "irritating mat- "ter in the firſt paſſages,"—to "acrimony which is "to be diſcharged on the ſkin,"—to "ſympathy†,"

* Practical Enquiry, p. 214, 215.

† This ſavours a little of the manner of the ancient writers.—"Fit "fingultus á repletione, vel inanitione, aut ab acerrima materia, vel "quod contagione a nervoſis partibus in ſympathiam ventriculus traha- "tur, vel a vicinis, ut ab hepate."

and

and laſtly, to "the difficulty with which venous "blood is returned to the heart, from the right "ventricle and the pulmonary artery being obſtruct-"ed."

"In * the multitude of counſellors there is "ſafety." In this numerous aſſemblage of cauſes there is at leaſt ſome chance of being right. But I muſt take the liberty of reminding you of your own quotation from Dr. Crauford, "by † the rules "of philoſophizing, we are to admit no more cauſes "of natural things than ſuch as are both true and "ſufficient to explain the appearances," and of the ſubſequent remark‡, which, if attended to, might poſſibly have directed to a plain and obvious indication that would have prevented the fruitleſs labour of "*ſpeculating in complex cauſes.*" But ſo true is the axiom, elegantly conveyed in poetic language:

——"There are who teach,
"Truths they not feel, nor practiſe what they preach§."

* Proverbs, chap. xi. v. 14.

† Exper. on Anim. Heat, p. 366, quoted by Dr. Bree.

‡ "Nature does nothing in vain, and having exhibited to us any "one cauſe of an operation, we are not fruitleſſly to ſpeculate on com-"plex cauſes of that effect." Practical Enquiry, p. 85.

§ The beautiful little poem, from which theſe lines were quoted, (the compoſition of Mr. WESTON, of SOLIHULL) is a deſerved panegyric on the public virtues, and private worth of the Reverend JOHN JAQUES, Rector of PACKINGTON, in WARWICKSHIRE; of whom it might be ſufficient to ſay, that he is juſtly entitled even to the poliſh-

ed

Certainly, however, it is more commendable, for us to content ourſelves with reaſoning from effects*,

ed praiſe of Mr. WESTON's pen: I will, however, add,—that this good and venerable man has fulfilled every duty of a diligent and faithful Paſtor, for nearly half a century; and ſo entirely poſſeſſed himſelf of the love and admiration of his pariſhioners, that all thoſe jealouſies and animoſities, all thoſe bickerings and altercations which diſturb the harmony of ſociety, and not unfrequently, end in ruinous litigation, are inſtantly ſubdued by the influence of his mild mediation. Eminently diſtinguiſhed in the deſk and the pulpit; he is equally celebrated for ſuavity of manners, evenneſs of temper, and judicious liberality.

"Amid the fold who thy protection ſhare,
"Does one, one lurking ill elude thy care?
"Does lacerated friendſhip mourn? does grief,
"Or age, or want, or ſickneſs aſk relief?
"The good Samaritan allays the ſmart,
"Binds up the wound, and heals the broken heart!"

WESTON's POEMS.

While I am thus adding my feeble tribute of applauſe to the general voice of that neighbourhood, of which Mr. JAQUES is ſo brilliant an ornament, I am tempted to digreſs ſtill further, by introducing the name of the illuſtrious EARL of AYLESFORD, founder of Packington church. In evil days (like the preſent) a bright example cannot be too often nor too highly held up to the admiration of mankind. A Peer of the Realm, who confers honour on his illuſtrious birth and exalted rank, by virtues "all his own," is not ſo commonly to be met with, as were "moſt devoutly to be wiſhed."—But when it is generally known that this nobleman (noble in every ſenſe of the word) beſtows that time and attention on elegant amuſements, and works of benevolence, which ſo many OTHER LORDS devote to the race-ground, the gaming-table, or to the celebration of the nocturnal orgies of folly, diſſipation, and vice; the torrent of democratic ſlander, and phlebeian abuſe of the ariſtocracy, muſt be, in ſome meaſure, ſtemmed. Would to GOD that ſuch a pattern of affability, integrity, charity, and piety, were generally imitated by the great!—The perturbed ſpirit of inſubordination, which now ſtalks through the land, "making night hideous," would then be charmed to reſt! and the hydra of ſedition, with all its forked tongues, would hiſs in vain!

* Lewis Cornaro's Treatiſe on a Sober and Temperate Life, p. 81.

with

ſuch as they appear, and with faithfully recording the ſymptoms of a diſeaſe, than that we ſhould hazard the propagation of error, by *attempting to account for all* thoſe effects and ſymptoms.

I am again ſo unfortunate as to differ from you on the cauſe of the paroxyſm taking place in the night. Every one knows that when the reſpiratory muſcles have been for ſome time at reſt, the ſerous effuſion into the veſicles of the lungs is increaſed, and that it will " create * more embarraſſment as " its bulk extends:" but you are pleaſed to remark, that " volition † being ſuſpended, the powers of " ſenſation and irritation are more active;" and quote Dr. *Darwin*'s aſſertion, " Our irritability to " internal ſtimuli; and our ſenſibility to pain or " pleaſure is not only greater in ſleep, but increaſes " as our ſleep is prolonged,‡" from which you infer, that the increaſe of ſenſation and irritation occaſions the effort to expel the offending matter.

If, however, I ſhall diſprove the premiſes, the concluſion which has been deduced from them, will neceſſarily fall to the ground.

Great names ſhould not deter rational creatures (although not poſſeſſed of the tranſcendent abilities

* Practical Enquiry, p. 222.

† Ibid.

‡ Zoonomia, vol. 1. Sect. xviii. quoted by Dr. Bree.

of

of the celebrated author of Zoonomia) from a careful inveſtigation of the doctrines delivered to us, even by authors of the moſt diſtinguiſhed talents, or the higheſt profeſſional celebrity. Error has been frequently ſanctioned by the reputation of great men; but it ſtill continues to be *error*, unchanged in its nature and effects, by the moſt general reception which the world may have been induced to give it, in conſequence of a too ready ſubmiſſion to the dogmas of authority.

If our ſenſibility be increaſed during ſleep, the mind ſhould ſeem not only to be perfectly awake when the body is at reſt, but to be more active in all thoſe functions which enable it to participate in the delight of refined ſenſations.

This reaſoning being applied to the immediate object of your enquiry, is thought to prove that the difficulty of reſpiration, which occurs at the commencement of the paroxyſm of Aſthma, is occaſioned by the irritation of the offending matter being more acutely perceived, at that particular period, when this effect takes place.

Let us reflect, for a moment, on what happens during the ſtate we call ſleep; a ſtate marked by the abſence of volition.

In ſleep, the judgment is undoubtedly ſealed up, and remains entirely dormant*; for the utmoſt

* Fordyce.

abſurdities

abſurdities of the imagination are allowed to wander without reſtraint, and the moſt diſcordant ideas are jumbled together, without any interference of the judgment to correct them.

Prolongation of time, and diſtance of place, are alike diſregarded: and a perſon may dream of a combat of Roman Gladiators in the drawing room at St. James's; or of Æſculapius in the ſhape of an owl, gravely ſeated at a Gulſtonean lecture, without any effort of the judgment being arouſed, by the abſurdity of the idea. But this is not the caſe when we are awake; the judgment *then* revolts* at ſuch incongruities, and although the ſportive activity of the imagination may preſent fallacious pictures, or heterogenious combinations of objects, the judgment inſtantly rejects them.

The imagination may be excited, either by the perception of ſtimuli, or by the aid of memory. Memory, which has been defined "the † power of "recalling ideas which have been formed by im-"preſſions made on ſome of the organs of ſenſe," may, itſelf, be arouſed by a new perception, ariſing from an impreſſion, ſimilar to what has before called it into action; and, as it is capable of being acted upon, by *every* new combination of perceptions, which the imagination may effect, it neceſ-

* Fordyce.

† Fordyce's Third Diſſertation on Fever, p. 28.

farily follows, that its action is, at least in some degree, regulated by that operation; which itself depends on the impressions conveyed by the corporeal organs of sensation, and on the ideas which have been laid up in the memory.

The imagination and the memory then, concur by an united action, to unfold and to prepare the ideas arising from the perceptions which the former receives from the organs of sense, and which the latter has treasured up; in order that the judgment may compare them together, and determine or decide on the propriety of their arrangement. The imagination resorts to the memory for a set of ideas, which the latter has the faculty of recalling. The memory assists the imagination in distributing, changing, separating them, and in recombining them after they have been separated. The memory and the imagination, therefore, may act, without * the judgment; although the judgment cannot exert itself without their assistance.

It has been admitted that the memory may act by its own inherent powers, but my present design is to consider it, as called into exertion by the perception of new ideas, in consequence of, and the cognizance of sensations resulting from the application of *external* stimuli.

* Fordyce.

The

The ſtronger, or more active the ſtimulus be, the force of its impreſſion will be proportionable; and the remembrance of it the more diſtinct. Great pain and great pleaſure, with all their concomitant ideas, ſink deep into, and are firmly engraven on the tablet of memory. *The acuteneſs of original perception, therefore, may be traced by the faithfulneſs of memory*, which is evidently influenced by it.

If Dr. Darwin's aſſertion be underſtood literally,—that our ſenſibility to *internal* ſtimuli is increaſed during ſleep;—that, as the memory does ſometimes act, independent of any excitement *from without*, and as the operations of the imagination, during ſleep, are uninterrupted by the exertions of the judgment, the ſenſations of pleaſure or of pain *ſo* produced, are *more acute* then, becauſe they are *unreſtrained;—in this caſe one might be not a little ſurpriſed, that ſuch a remark ſhould have been adduced, in order to ſupport the opinion, that *when volition is ſuſpended, our perception of ſerum accumulated in the bronchia is moſt acute.* But notwithſtanding the *literal* meaning of the words "internal ſtimuli," and the further elucidation of that expreſſion, in the ſecond volume of Zoonomia, where the learned author ſays, "in † delirium, reverie, and ſleep, the power of perception is *aboliſh-*

* Lord Monboddo's Ancient Metaphyſics, vol. 2. book iv. C. 4 & 5.

† Darwin's Zoonomia, vol. 2, p. 141.

 "*ed*,"

"*ed*;" and mentions "Incubus," as probably "owing * to the irritability of the ſyſtem being *too* "*ſmall* to carry on the circulation of the blood "through the lungs, during ſleep, when the volun-"tary power is ſuſpended:" it may be preſumed, from other expreſſions, ſuch as that of "the ſyſtem "being more ſenſible † and more irritable during "ſleep," and a "perpetual ‡ increaſe of ſenſibility "during the continuance of ſleep, as in ſome inſtan-"ces of epilepſy, *aſthma*, &c."—that you have Dr. *Darwin's* authority on your ſide, to ſupport a theory, or rather an opinion, which is contradicted by all ‖ experience, and by almoſt every metaphyſical writer, from the days of Ariſtotle and Plato in-

* Darwin's Zoonomia, vol. 2, p. 400. Whether the doctrine here advanced be true or falſe, is not the object of my enquiry, the reference being made only to prove the REAL meaning of Dr. Darwin's expreſſion reſpecting the INCREASE OF SENSIBILITY DURING SLEEP.

† Darwin's Zoonomia, vol. 2, p. 399.

‡ Ibid. p. 515.

‖ Shall it be aſked? How is it poſſible for a ſuſpenſion of the power of volition, to DIMINISH the velocity of the circulation through the lungs; and, at the ſame moment, to increaſe our ſenſibility of, and conſequently our efforts to expel, THE OFFENDING MATTER? which are manifeſtly contrary effects. It is true, Dr. Darwin has SAID that "the ſyſtem becomes more ſenſible, and more irritable during ſleep;" and in another place, that the degree of irritability, which exiſts when volition is ſuſpended, is inſufficient to carry on the circulation with its accuſtomed energy: and in another part of his work, that "in deli-"rium, reverie, and ſleep, the power of perception is ABOLISHED."

If the irritability be too ſmall to maintain the circulation in its uſual force, how can it occaſion an INCREASED effort to expectorate the accumulated muçus? In ſhort, how are the above contradictions to be reconciled?

No

cluſive. Neither his authority, however, nor that of all the Phyſiologiſts in Europe, ſhall make me acknowledge what is ſo directly contradicted by every one of the ſenſes; nor will I ever conſent to believe, that the wiſe Author of our being only ſent us into the world *to dream*, until the advantages which reſult from ſuch an increaſe of ſenſibility during ſleep, ſhall be more evidently demonſtrated.

Can it be ſuppoſed that our perceptions ſhould have been rendered moſt acute, at the time when we are leaſt capable of arranging, or of enjoying the arrangement of the ideas which they excite? or, that the wandering pleaſures of an uncontrolled imagination, full of abſurdity; and the wild excurſions of memory, without connexion, ſhould have been beſtowed on us, as the ſupremeſt gratification which we are able to feel? Had this been the caſe, the martyrdom of the ſeven ſleepers, ſaid to have been ſhut up in a cave at Epheſus for more than three hundred years, would, after *ſo much pleaſure*, have been almoſt enviable!

It is, therefore, *unreaſonable to ſuppoſe* that our ſenſibility is increaſed during ſleep; and the *belief* of it may be proved to be erroneous, by the following facts.

No authority ſhould be regarded in medicine but that of juſt theory, founded on real obſervation. No conſequences ſhould be admitted † which are not immediately derived from actual experience, and no ſubmiſſion is due to nominal influences.

† Lavoiſier's Traité Elementaire de Chimie, vol. 1.

A great

A great proportion of our ideas may be ſaid to originate through the medium of ſight, and unleſs the mind be very attentively * engaged, the objects painted on the retina make ſome degree of impreſſion. It is not neceſſary that this impreſſion ſhould continue for a long time, in order to its being retained *in the memory:* for a comet may be ſeen for a moment, and the perfect image of it never be effaced from our recollection. It is only neceſſary that the mind ſhould be *capable* of being fully impreſſed at the time.

A complete picture may be formed, conſiſting of many objects;—theſe are doubtleſs, all of them, repreſented on the retina at once; but the mind is only ſenſible of the moſt ſtriking features, and its attention being engaged by them, *all* the ſeveral objects are not conveyed to the memory; and the recollection † of many of them is conſequently loſt.

In ſleep, the eye-lids are uſually cloſed, but not always; for ſome perſons ſleep with their eyes wide open, and in broad day light. Volition being ſuſpended, the muſcles of the eyes ceaſe to accommodate themſelves ‡ to the diſtances, at which objects are moſt diſtinctly viſible; but there are

* Fordyce.

† Ibid.

‡ Dr. Alexander Monro's Treatiſe on the Eye, p. 137.
Dr. Brandiſh's Diſſertatio Inauguralis de Viſu, p. 39.
Reid's Inquiry into the Human Mind, p. 240.

ſome

ſome objects occaſionally brought to the focal diſtance*, and yet they will not be found, once in a million of inſtances, to excite the ſmalleſt degree of perception. Nor is any idea produced by the *conſciouſneſs* of light, which, when we are awake, accompanies our perception of objects, and is preſent when the retina is only ſtimulated by the rays of light falling on it:—the eye being placed in a ſituation in which the reflexion of other objects cannot poſſibly reach it. We may, indeed, *dream* of perceiving the impreſſion of light, but this is an act of memory † alone, nearly reſembling that ſtate of ideal inſanity ‡ in which a patient imagines he ſees or converſes with perſons, who have no external exiſtence to his ſenſes at the time.

Even if objects, at the focal diſtance, were *viſible in ſleep*, it would be no proof that the powers of perception *are then increaſed:* but as the diminution of ſenſibility in the optic nerve always accompanies the quieſcence of the voluntary muſcles, this is an additional argument againſt the doctrine advanced in your "Practical Enquiry."

If we look with attention at the ſtructure of the ear, we ſhall be convinced, that although the ſenſe of hearing be termed *involuntary*, there are parts of that organ which aſſiſt in the conveyance of ſounds

* Fordyce's third Diſſertation on Fever, p. 34.

† Fordyce.

‡ Arnold's Obſervations on the Nature, &c. of Inſanity.

to

to the auditory nerve, evidently controllable by the powers of the will*, which does not merely render *them* more diſtinct, by abſtracting our attention from other impreſſions, but manifeſtly contributes to increaſe their effects, by exciting the action of thoſe muſcles which fit the meatus auditorius for the primary reception of ſounds, and at the ſame time, adapt the membraneous part of the caſe† which contains the veſtibule and ſemicircular canals, to the impreſſions about to be made on the laſt named organs.

Many ſounds, undoubtedly, enter the ear while we are awake, of which the mind takes no cognizance; but it ſurely will not be contended, that we *hear better when we are aſleep:* nor will it be ſaid that the ſhepherdeſs, repoſing on a bank of violets and honeyſuckles, enjoys the *ſmell* of their delicious prefume, the more ſenſibly during her ſlumbers.

Epicures may ſometimes be gratified even by *dreaming* of turtle and veniſon; but if, by your means, the opinion that their *taſte* is improved during ſleep, were once eſtabliſhed, it would operate as an anodyne to the whole Court of Aldermen; who would *gape* at the name of Bree,

"thenn nodde ther thankes and falle aſleepe.‡"

* Darwin.

† See an excellent deſcription of theſe parts, by Dr. Monro, in his Remarks on Profeſſor Scarpa's Book on the Ear, p. 239.

‡ Chatterton's (or Rowley's) Account of Cannynge's Feaſt.

Another

Another organ of perception is the ſkin: and will any man in his ſenſes pretend to argue, that the effect of Cantharides, or of any other irritating application*, is felt *more acutely during ſleep?* To come ſtill nearer to the point; the accumulation of water in the painful aſcites, and in dropſy of the cheſt, and in hydrocele, is certainly not productive of any increaſe of diſtreſsful ſenſation during ſleep: neither are abſceſſes and tumours, nor is the ſenſe of diſtention in caſes of aneuriſm, or varicocele, felt more acutely at that time.

Theſe remarks might be extended to a great length, but you will *perceive* that I think it unneceſſary to advance further proof. The experience of every perſon in the world being found poſitively to contradict this *imaginary* increaſe of ſenſibility; I aſſume, then, that the fluid accumulated in the lungs, during aſthma, is *not* felt with increaſed ſenſation during ſleep, but only in proportion to its increaſe of quantity; and that *the powers of ſenſation and irritation are* NOT *more active* at that period.

* My much eſteemed friend, the late H. M. Eſq. repreſentative in parliament for Boſſiney, in Cornwall, having accuſtomed himſelf to read till a very late hour, after he retired to his chamber, happened once to fall aſleep during this employment, with a candle in his hand, which, being ſoon melted by the fire, fell on his leg; and the wick following the wax, continued to burn until his ſtocking was conſumed, and a conſiderable portion of ſkin perfectly coagulated by the heat, ſo as to form a large eſchar, before the ſenſation of pain (although the ſtimulus muſt neceſſarily have been acute) arouſed him from ſleep. Many ſimilar, and, perhaps, more ſtriking examples, might be produced, if neceſſary.

Your obſervation on the beneficial activity of the lymphatics, in the removal of offenſive matter from every part of the ſyſtem, (ſupported by the coincidence * of Dr. Darwin) having induced you to reject an opinion that "a *paralytic* † *atony*" of that ſyſtem is one of the cauſes of the accumulation of fluid in aſthma; I ſhall not introduce any further remark on that ſubject, than a hint that ſome pains appear to have been taken, to diſprove what had never been eſtabliſhed: for, (notwithſtanding Dr. Richter's ‡ Theory of Dropſy) there is no more *evidence* ‖ to ſupport the doctrine of "a *paralytic* "*atony*" of the lymphatics, in *uncomplicated* aſthma, than to prove the exiſtence of the man in the moon.

As to the puerile idea of the elective **, or diſcriminating power of the lymphatics, it is ſo directly repugnant to every principle of true reaſoning and phyſiology, as ſcarcely to deſerve the trouble of refutation; although I have heard an anatomical

* Darwin's Zoonomia, vol. 2, p. 163.

† Practical Enquiry, p. 273.

‡ A German phyſician, and author of a work entitled "Medical "and Chirurgical Obſervations," tranſlated by Dr. Spens.

‖ Let it be remembered that Dr. Darwin only SUPPOSES that the Paroxyſms of aſthma depend on what he calls "torpidity of the abſor-"bents," and acknowledges that their "cauſe is not WELL underſtood." "Zoonomia, vol. 2, p. 520.

** "It takes up by SELECTION."
Practical Enquiry, p. 276.

lecturer

lecturer gravely expreſs his aſtoniſhment at the *wonderful* alacrity with which theſe veſſels take up nutritious matter, and refuſe that which is noxious! Where is this *wonderful ſagacity*, when they ſuffer themſelves to be gorged with a mixture of quick-ſilver and greaſe, which they imbibe with as much avidity as milk and honey, or the chyle which ſupports their very exiſtence?

I paſs over the particulars of your diviſion of Aſthma into *four ſpecies*, the neceſſity of which, will, I preſume, be ſuperſeded, when I have deſcribed the *nature* and *cauſe* of that irritation, which, *in every ſpecies* of the diſeaſe, you have admitted to exiſt.

The earlier writers, it is well known, conſidered Aſthma as a pituitous affection, which was afterwards divided into two ſpecies, and termed Humoral and Nervous. The firſt, they deſcribed as occaſioned by a *defluxion* * *of ſerum from the head*, and the laſt has been ſince called a ſpaſmodic effort.

And although the ancients either "boldly waded "in darkneſs†" or were led into endleſs error by the glare of falſe theory; they ſaw plainly that a quantity of mucus was to be expectorated, and,

* "Congeritur in capite pituita, quæ labitur in imbelles jam pul-"mones." J. Heurnii in lib. iii Aphoriſm. Hippocratis, p. 210.

† Darwin's Zoonomia, Preface to Vol. I.

 therefore,

therefore, endeavoured to promote this operation; leaving the *real cauſe* of the accumulation, to be diſcovered by the induſtry of future ages. The methods employed were certainly different from thoſe which a modern Phyſician would recommend; and, perhaps, ſome of their remedies were made uſe of without much reflection or ſtudy. Thus, Emetics were preſcribed by *Riverius*, although he certainly did not conſider the irritation of the ſtomach as in any degree connected with the diſeaſe. Zacutus Luſitanus uſed Diuretics, but it is not very clear that he had the leaſt idea of depriving the blood of any principle which at all conduced to the production of Aſthma. *Willis*, it is true, ordered expectorants, (as *Horatius Augenius*, *Sennertus*, and *Riverius* had done before him) and appears to have had a confuſed notion of what he called a *ſerous humour* in one place, and a *vapour* in another, exiſting in the blood: he undoubtedly uſed expectorants becauſe of the mucus in the bronchia, but it is not ſo eaſy to determine his reaſons for that elegant addition, prepared from ſnails and earth-worms *.

* Snails and Earth-worms, however, as well as ALL THE REPTILES of the earth, have had their advocates, and even admirers; and I am not ignorant, that many and great virtues were formerly aſcribed to them; of which a very pompous record has been made. "Inſigniter diuretici ſunt, diaphoretici, anodyni; diſcutiunt, emolliunt, obſtructiones reſerant, lac augent, vulnera nervoſque præciſos glutenant. Uſus præcipui ſunt in Apoplexiâ, Spaſmo, aliiſque affectibus nervorum ac muſculorum—In ictero, hydrope, dolore colico. Imprimis autem ſpecifice proſunt in arthritide ſcorbuticâ, podagros dolores leniunt." Dale's Pharmacologia, p. 530.

More

More modern practitioners have described the particular condition of the body, in which Asthma makes its appearance: and, from an attentive consideration of it, have discovered, that, in order to prevent the recurrence of the disease, that state of the system in which it has been most frequently observed to arise, must be prevented.

Hitherto, however, I believe the manner in which that condition of the body, which has been called predisponent to Asthma, really produces the disease, has not been explained, nor even attempted to be explained, by any author who has expressly written on disordered respiration. It is amusing to trace the regular, but often, slow progression, by which genius is conducted, from one discovery to another, and so on; 'till suddenly she shall sit down, as if impatient of a resting place, at the very threshold of some important improvement; contented with the gleanings she has already met with, and seeming to disdain the trouble of further investigation.

In later times, Practitioners having acquired a correct idea of the arterial system, and finally of the lymphatics, were no longer at a loss to determine in what manner the serous effusion, which obstructed the bronchial tubes, was deposited in the lungs; nor to account for the invisible removal of it, in various instances; and the translation of

the

the diſeaſe, or rather of this ſymptom of it, to ſome other cavity.

They have recorded the appearances; they have obſerved the changes which take place in the ſyſtem, at the time of the occurrence of the diſeaſe; and at the periods of its variation: they notice the irritating effect of the effuſion, on the fine membrane which lines the branches of the trachea; they include the diſorders of the firſt paſſages, among the cauſes which induce ſuch a ſtate of the body, as particularly fits it, for the attack of the paroxyſm; they even mention the *acrid* quality, as well as offending quantity, of the effuſion; yet not one among them, not even you yourſelf, have thought it expedient to enquire, what is *the nature* of this irritating quality, in the ſerum? and what the means by which it is produced?

You admit, Sir, that the influence of this principle is ſo powerful, as to be capable of exciting violent ſpaſmodic contractions of the muſcles concerned in reſpiration, and do not ſo much as hint at the probability of this irritating quality having been acquired, ſubſequent to the exudation of the ſerum: it may be preſumed, therefore, that you agree with preceding authors in ſuppoſing ſuch quality to have exiſted in that fluid, previous to its being expelled from the blood veſſels: and yet, you have not thought it worth while to enquire into its properties, and the manner in which it is formed!

Viewing

Viewing the ſubject in this light, I am almoſt induced to ſuppoſe, that they, who have trodden in the ſame path before me, had become weary, by the fatigue of their reſearches, and abandoned the purſuit, after having followed the object of their enquiry, through its greateſt intricacies, and chaſed it into open day light.

The proximate cauſe of Aſthma, is allowed to be a ſerous effuſion in the pulmonary veſicles, offending, either "by * its oppreſſive bulk, or acrid "quality, or both."

Hence it may be inferred, that you do not attribute the offence to the ſtimulus of *quantity*†, ſimply conſidered as ſuch; becauſe, it may be produced, when no evident proof of the preſence of groſs matter can be demonſtrated: and we can ſcarcely conceive how an effect may be occaſioned by quantity only, when it is ſo minute as to be imperceptible.

* Practical Enquiry, p. 318.

† "The ſecond ſpecies of irritation is the acrid or offenſive QUA-"LITY of ſome UNOBSERVED matter, CONVEYED BY THE AIR, "and attached in the act of inſpiration to the SENSIBLE membrane "lining the trachea and bronchial pipes." Practical Enquiry, p. 308. Two obvious queſtions ariſe out of this ſhort ſentence: firſt, What proof is there of this UNOBSERVED MATTER being conveyed by the air? Secondly, How is the SENSIBILITY here allowed to the membrane of the trachea reconcilable to YOUR PRECEDING REMARK, that "the lungs have little ſenſation;" which has been already noticed in the fifth page.

The

The idea of quantity, connects with it that of ſpace, or diſtention; when, therefore, an effect is produced by matter, which ſtimulates in a very ſmall quantity, the mind refers that effect to a ſpecific power of action different from quantity, which we call *quality*.

Nevertheleſs, it is neceſſary that a certain number of original points, or atoms, ſhould be collected, in order to excite perception: but the increaſe of their number does not alter their quality, it only augments the force of their effects.

A maſs of matter, uniformly red, being divided * into its minuteſt parts, loſes its *ſenſible* colour, but every particle is ſtill red, though it be too ſmall to impreſs the organ of ſight with that idea. A particle of red blood is not altered in its original quality by being divided, only, it no longer retains its uſual appearance. If a maſs of matter, by having its parts divided, were to ſuſtain any alteration of its nature, or in another word, of its quality; it would alſo acquire *new* properties by an accumulation of ſize; but, a ſingle grain of ſalt conveys a perfect ſenſation, not at all different, except *in degree*, from ten thouſand grains; for an original particle of matter, muſt, for ever, continue † the ſame, and remain eternally unalterable in its eſſence; un-

* Treatiſe on the Digeſtion of Food, p. 125.

† Fordyce.

leſs

leſs the Almighty, who created it, ſhall be pleaſed, miraculouſly, to give it different properties, or to annihilate it.

But matter, which, in a ſmall quantity, is perfectly inert, and incapable of exciting the action of the living fibre; may, when accumulated to a certain degree of bulk, irritate by its power of diſtenſion. In this caſe, the effect may be ſaid to ariſe from *quantity*.

To illuſtrate my remark. An ounce of warm water thrown into the ſtomach, does not ſtimulate it to contract, in any degree, of which we have the ſmalleſt evidence or perception:—but a quart, or, perhaps, a third or fourth part of that meaſure, will excite violent contraction. Here, then, the ſtomach is excited to action by the ſtimulus of quantity, or diſtenſion. It is evident that no new properties are, in this caſe, applied to the ſtomach by repeating the draughts of water; for, if the quantity, which is known to be ſufficient to provoke vomiting, be divided into ſmall portions, and ſwallowed at conſiderable intervals, although the ſame quantity will come in contact with the coat of the ſtomach, in the latter caſe as in the former; the ſame effect will not be produced. Neither is there any thing in warm water, which has a tendency to occaſion the contraction of the living fibre:—it does not corrugate the ſkin when applied to it, and certainly does

not ſtimulate a muſcle to contract, by any inherent property, notwithſtanding it has been termed *a cordial**. Its effect, then, muſt depend on the diſtenſion it produces, partly by its mechanical bulk, and partly by the expanſion which its heat may occaſion, of the air, with which it happens to meet in the ſtomach.

Again, a particle of cantharides, or of volatile alkali, when applied to any part of the living body, ſtimulates it, and produces pain, or at leaſt a ſenſe of heat; but certainly not from its weight or power of diſtenſion. Many particles, will, it is true, produce a proportionably greater effect;—but in no way different, unleſs in degree, from that of a ſingle particle. This effect, is ſaid to ariſe from the peculiar quality, or property, with which theſe ſubſtances are endued.

In like manner a few grains of ginger, or pepper, being applied to the membrane of the noſe, induce the effect called ſneezing, which is an effect of quality, not of quantity; for an equal portion of ſtarch, or of powdered chalk, might be ſo applied, and yet no ſuch effect be produced. A perſon, indeed, accuſtomed to the uſe of ſnuff, becomes habitually inſenſible to the ſtimulus of the ſame quantity of it, which is capable of exciting violent efforts in ano-

* Boerhaave's Praxis Medicinæ.

ther:

ther: but this difference is manifeſtly occaſioned by a morbid affection of that part of the membrane to which the ſnuff has been cuſtomarily applied; for any other living fibre of the ſame perſon may be irritated by an equally minute particle of ſtimulating matter, which would be required to produce an effect on another perſon; and he who is become inſenſible to the action of a particular quantity of any ſtimulant, may yet be affected by a greater number of particles of the ſame, notwithſtanding it has been ſhewn that no difference in *the nature* of their action is occaſioned by increaſing their quantity.

Matter therefore which, in minute particles, is perfectly incapable of ſtimulating, does not acquire any *new properties*, in order to be rendered capable of exciting irritation in accumulated quantities.

Hence it appears,

I. That matter perfectly incapable, in its nature, of irritating the bronchia, or of exciting contraction of the reſpiratory muſcles, may be depoſited in the lungs, without offending, unleſs by its quantity.

II. That matter of an irritating quality may ſtimulate the veſſels, in which it is contained, although it be preſent in a quantity ſo ſmall as to eſcape detection by the eye.

In every caſe, in which a conſiderable alteration of the quality of the ſolids and fluids of the body is produced (and, as theſe are compound ſubſtances, they may ſuffer alteration, either by a variation in the proportions of their component parts, by being entirely deprived of one or more of them, or by having ſome new principle, different from any which they before contained, ſuperadded to them) a certain degree of diſorder, or derangement of their functions, is neceſſarily occaſioned. Symptoms of ſuch irregularity preſent themſelves to an attentive obſerver, during life; and after death, an affection of the ſolid parts, exhibits on diſſection, appearances which are called morbid; becauſe they differ from the uſual appearances of the ſame organs, when in an healthy ſtate.

It is by an attention to the phenomena of theſe ſymptoms, during life, and of theſe appearances, after death, that we are enabled to diſcover the nature and the cauſes of thoſe irregularities which conſtitute diſeaſe: and to adapt remedies ſuitable to their removal when preſent, and their prevention when abſent.

It immediately ſtrikes the moſt ſuperficial obſerver, that the preſence of a ſerous effuſion in the lungs, is not a neceſſary conſequence of their particular organization; and, therefore, that it muſt be occaſioned by ſome derangement, either in that organ,

organ, or in thoſe parts of the ſyſtem to which it is ſubſervient, or with which it is directly connected.

Anatomy has taught us the nature of the connexion between the lungs and the reſt of the ſyſtem. It is ſufficient for my preſent purpoſe to obſerve, that the lungs are furniſhed with a ſet of tubes, called air veſſels, (the ſtructure of which it is unneceſſary for me to inveſtigate in this place) whoſe large trunk opens to admit atmoſpheric air, but is fitted with a * contrivance for the complete excluſion of any other matter;—and whoſe extremeſt ramifications open into veſicles which every where accompany the terminations of another ſet of veſſels proceeding from the heart, and carrying red blood, with which the air, admitted by the bronchial tubes, is intermixed. This operation is perpetually going on, and the arterial blood, having received a portion of the atmoſpheric air, is re-

* The Epiglottis.

The muſcles * which ſhut up the paſſage of the larynx are ſo ſtrong† in many perſons, that they can ſuffer a large quantity of fluid to paſs down the throat, without any danger of the ſmalleſt particle being admitted into the trachea, although the mouth be kept wide open.

Cuſtom undoubtedly increaſes the ſtrength of this faculty, as appears in the example of ſome of the eaſtern nations, where it is a common cuſtom to pour their drink into the æſophagus, without touching their lips with the veſſel which contains it.

* Cheſelden's Anatomy, p. 78, and ſubſequent.

Winſlow's Anatomy, tranſlated by Douglas, Vol. 2. Sect. 10.

† Cowper's Anatomy by Albinus—Tab. 5th of the Appendix.

turned

turned by other veſſels, called veins, to the heart, to be diſtributed to the remoteſt parts, as well as the inmoſt receſſes of the body, while certain particles, which may be termed excrementitious, having been parted with, by the blood, in exchange for pure air, are received into the bronchial veſſels, and by them conveyed out of the body in the ſhape of vapour, which is vulgarly called *breath*.

Beſides this vapour, the air veſſels in a ſtate of health, throw out no other matter: but in the diſeaſe termed Aſthma, the muſcles which alternately dilate and contract the cavity of the thorax, and fit the lungs for receiving the air, and reſpiring the breath, through the bronchial veſſels, are violently irritated, and with laborious efforts expel, through the ſame tubes, a quantity of mucus, which, it is obvious, could only have entered into their cavities, through the veſicles and the minute extremities of the blood veſſels.

From the ſize of their orifices, it is equally obvious that this mucus, at the time of its paſſing through them, muſt have been of no very conſiderable denſity: and that it had previouſly circulated in the blood veſſels, either in combination with the blood itſelf, or in a ſeparate ſtate.

Innumerable opportunities of obſerving the blood of patients, labouring under the paroxyſm of

Aſthma,

Aſthma, having never afforded the leaſt ground of ſuſpicion, that this fluid had exiſted in the blood veſſels ſeparate from the blood; it neceſſarily followed, that it muſt have been combined with the blood itſelf, and conſequently, that it was originally parted with, in the form of ſerum.

An attentive examination of the delicate membrane, which lines the branches of the trachea; and frequent experience of the diſtreſs, occaſioned by any extraneous matter, even in the ſmalleſt quantity, being forced into them, together with the appearances on the diſſection of healthy ſubjects, were ſufficient to convince modern practitioners that the effuſion diſcovered in Aſthma, could only be derived from the arteries of the lungs; but it would naturally lead them to remark, that matter of the moſt unirritating kinds, and ſuch as produced no ſtimulating effects whatever, in another part of the body, might occaſion great irritation there, however ſmall might be its quantity.

If then they had detected an acrid quality in the ſerum, effuſed into the lungs, it would have been difficult to ſay whether that organ might not have been irritated by the ſtimulus of quantity, rather than of quality.

But as no intermixture of the ſerum with any ſubſtances, but the ſubſtances of which the atmoſpheric air is compoſed, could poſſibly have taken

place,

place, after its exudation in the lungs, if any acrid quality prevailed in that fluid, it muſt either have been obtained previous to its being depoſited there; or have acquired it, by attracting oxygen from the atmoſpheric air, and forming an acid by combining with it.

The next obſervation which occurs, is—that if this acrid matter exiſted previouſly in the blood veſſels, it muſt have been capable of uniting with the blood, without producing any apparent alteration of its texture, when examined out of the veſſels.

But, although the bronchial veſſels are extremely irritable, ſo that the minuteſt particle of extraneous matter may be ſufficient to ſtimulate them; yet, as the veſicles in which they every where * terminate, *may be* endowed with a leſs degree of ſuſceptibility than the larger branches, it is poſſible that a certain accumulation may be permitted, before the effuſion becomes capable of exciting the action of the reſpiratory muſcles to expel it. The *nature* of the effect, when exerted, is, however, preciſely the ſame.

In the hiſtory of Aſthma, it ſeems to have been admitted, that the effuſion may produce irritation, " by its oppreſſive bulk † or acrid quality,

* Winſlow's Anatomy, Vol. 2. Sect. 9. p. 105. 137.
Cheſelden's Anatomy, p. 173.
† Practical Enquiry, p. 318.

" or

"or both," and yet its exclusion from the blood vessels has been referred to a particular state of the arterial system, unconnected with either of these circumstances: for you have deliberately assigned *a morbid and * atonic state of the pulmonary capillaries, as the cause of the effusion of serum.*

Serum is undoubtedly expectorated, often, in considerable quantities, by persons who sustain the paroxysm of Asthma. It is discovered, often, in considerable quantities, in the bronchia of those who are cut off by the violence of the disease:—but, although the stimulus of quality, as well as quantity, is not denied, an *imaginary* state of the blood vessels has been resorted to, in order to account for the expulsion of the fluid from them, rather than it should be referred to either of the causes, which are acknowledged to influence the bronchial vessels, in the expectoration of it afterwards.

For these accounts of the relaxation and debility of the exhalent arteries of the lungs, we are indebted to those writers, who,

> "Nobly deserting common sense,
> "For metaphysic excellence,"

have thought it proper that they should *attempt to account* for every circumstance † in the history of

* Practical Enquiry, p. 157.

† "The coldness of the lower limbs proceeds from THE DEFICIENCY OF ANIMAL HEAT; and the IRRITABILITY of the mind "from the anxiety of the præcordia."

Practical Enquiry, p. 235.

Aſthma; and who did not remember that *all our Theory had better periſh*, than that "*one jot or one* "*tittle*" of it ſhould reſt on the unſtable foundation of conjectural hypotheſis.

It has been remarked, that every conſiderable alteration in the fluids, as well as in the ſolids of the body, is accompanied by correſpondent ſymptoms. Now, it will not be denied that the ſtate of the pulſe varies, with every change in the quantity, if not in the quality of the blood, as well as of the rigidity or relaxation of the veſſels which contain it. We have only, then, to reſort to this faithful index, in order to diſcover what is the particular ſtate of the veſſels during Aſthma, and what changes have taken place in the circulation.

Whenever the quantity of the circulating fluid be diminiſhed, the beat of the pulſe * will neceſſarily be ſmaller, in proportion to the meaſure of blood thrown out by the left ventricle: and whenever there be an increaſe of quantity, a fuller meaſure being expelled from the heart, will conſequently occaſion a proportionable increaſe of the ſenſation of fulneſs in the pulſations.

Theſe varieties will obtain, whether the arteries, which contain the blood, be ſtrong or weak, rigid or relaxed:—for the pulſe may be hard and

* Fordyce.

ſmall,

ſmall, or ſoft and full, or hard and full, or ſoft and ſmall, at the ſame time; for, as the fulneſs and ſmallneſs depend on the contained fluid, ſo the hardneſs and ſoftneſs are influenced by the ſtate of the containing veſſels.

It is unneceſſary for me to contraſt the feel of fulneſs with that of contraction, an evident ſtate of the arteries on particular occaſions; I proceed, therefore, to mention other ſenſations, called ſtrength and weakneſs, which are attributable, in moſt caſes (though, perhaps, not in all) to the veſſels; and increaſed and diminiſhed celerity, which are controlled by the frequency of the contractions of the ventricle; and may, therefore, take place, when the arteries are either rigid, ſo as to give the ſenſe of hardneſs, or relaxed, ſo as to occaſion the feel of ſoftneſs at the ſame time*.

The pulſation of the arteries may be increaſed in frequency by any muſcular exertion which propels the blood with greater rapidity than uſual to the heart. It may alſo be accelerated by other cauſes, when the muſcles are not excited to action.

* This account of the pulſe appears to be ſufficient for the purpoſe intended in the preſent argument: but, as it is of great importance that the varieties of the circulation be minutely attended to; I ſhould have been more particular in this part of my letter, if I did not entertain ſome intention of publiſhing a Tract, (lately compoſed, at the entreaty of a medical ſtudent, to be read as an inaugural diſſertation, on taking a Degree in Phyſic) which contains a general hiſtory of the circulation, with all its phenomena.

In fever, while the body remains perfectly at reſt, and the paſſions of the mind unagitated; the pulſe ſometimes increaſes to an hundred and fifty ſtrokes in a minute, or even ſo as not to be counted. Fever, however, as I have before obſerved, by no means depends on this frequency. I have attended patients in Typhus Fever (ſuch as I have endeavored to deſcribe in a ſhort Eſſay*, lately publiſhed) where the ordinary number of pulſations varied very little from the healthy ſtandard; and, in the month of July laſt, the pulſe of Mr. *Smith*, of *Jacknett*, near *Knowle*, in this county, remained exactly at 73 in a minute, for at leaſt ſome hours every day, for ſeveral days during the continuance of a ſimilar diſeaſe; and at the ſame time when the dark fur on the tongue with petechiæ were preſent.

In many other diſeaſes, as well as fever, and in many of the paſſions of the mind, great increaſe of frequency in the pulſe, is obſervable, without any exertion of the muſcles.

The celerity of the circulation may, therefore, depend either on the heart having acquired an increaſe of ſuſceptibility, or on the blood having gained an increaſe of the power of ſtimulating: for, the heart is not merely excited to contract on its contents, by the ſtimulus of diſtention; elſe in thoſe perſons who have the *foramen ovale* remaining open,

* Eſſay on the Nature and Treatment of a Putrid Malignant Fever, octavo, Rivingtons, London.

the

the circulation would not ceaſe *ſo ſoon* as it is known to do, when the action of the lungs has been ſuſpended.

It would be ſuperfluous for me to enlarge upon the ingenious experiments, already ſo well known, which have illuſtrated the manner in which the oxygen received into the blood, from the atmoſpheric air, is conducive to the maintenance of that perpetual ſtimulus which excites the heart to contract on its contents.

The heart and arteries, may probably often differ in their relative * proportions of ſtrength; and the heart, itſelf, may, under ſome circumſtances, be *more* diſpoſed to contract, that is, more ſuſceptible of ſtimulus than at other times, like other living fibres of the body; and it may alſo become *leſs* capable of ſuffering irritation. It is ſufficient, however, for me to conſider it, at preſent, as influenced to contract, by the *quality* of the fluid paſſing through its cavities.

Now, it cannot be denied, that the circulation frequently becomes more rapid, without any increaſe of hardneſs or ſoftneſs, of ſtrength or weakneſs, which manifeſtly depend on the veſſels; theſe ſenſations, therefore, not neceſſarily accompanying the increaſed celerity, evidently prove, that the latter depends on the increaſed frequency of the

* See Dr. Cullen's Treatiſe on the Materia Medica, Vol. 1.

contractions

contractions of the ventricle, excited by the peculiar property of the circulating fluid, and not occasioned by any alteration in the heart itself.

If it be admitted that the blood does stimulate the ventricle by its quality, and that that quality may allow of variation; it necessarily follows, that the ventricle may be stimulated to more frequent contractions, whenever the blood contains a greater proportion of irritating power.

How, then, can it be asserted, that *the heart is insensible to every stimulus but that of the blood, possessing its healthy* * *qualities?*

After this view of the subject, I do not think it worth while to trouble you with any remarks on Baron Haller's *assurance*, "that † the heart has very "few nerves;" nor on M. *Behrends's* opinion on the same subject.

* Practical Enquiry, p. 237.

† "Haller ASSURES us, that the substance of the heart has very "few nerves; and M. Behrends ALLOWS a still more inconsiderable "number, and those of TRIFLING magnitude."

Practical Enquiry, p. 237.

With regard to the LATTER opinion, as the work from which it was extracted is not referred to, in the "Practical Enquiry;" it may not be improper to remark, that it is only the sentiment of a STUDENT, delivered in an academical exercise at a foreign University:—See "Differt. Inaug. Anatomico-Physiologica quâ demonstratur cor Nervis carere: additâ disquisitione de Vi Nervorum Arterias cingentium. "Auct. J. B. J. Behrends."

I come

I come, in the next place to, conſider the ſtate of the pulſe during Aſthma; and its indications.

One condition of the pulſe only, appears to have been noticed in the firſt part of your "Practical "Enquiry:"—it is the quickneſs*, unaccompanied by hardneſs, which prevails at the commencement of the paroxyſm. I am not to enquire how it has happened, that the varieties of the circulation have been ſo little attended to: but ſhall endeavour to ſupply what appears to be a material deficiency, in the beſt manner I am able.

At the attack of a paroxyſm of Aſthma, a degree of fulneſs is commonly obſervable in the pulſe, accompanied with ſome increaſe of celerity. This fulneſs is not always proportionate to the quantity of mucus to be *expectorated*; but, I believe, is always exactly regulated by the quantity of ſerous effuſion.

The fulneſs, as well as the quickneſs, depends on the ſtate of the blood itſelf, and is clearly unconnected with the condition of the arterial ſyſtem.

The indication of the former cannot be miſunderſtood; the latter evidently points out an increaſe of ſtimulating power.

* Practical Enquiry, p. 79.

When

When the living principle is unuſually exhauſted by muſcular exertion, the blood is hurried with increaſing frequency to the lungs, that it may receive a larger ſupply of oxygen: but when the muſcular efforts ceaſe, the blood no longer requiring ſuch an extraordinary quantity, the circulation becomes more tranquil, and the pulſe leſs frequent.

But, in Aſthma, where no muſcular exertions have taken place (I mean before the extravaſation of ſerum has been effected) we find the pulſe increaſed in frequency.

This frequency, then, muſt depend on ſome cauſe, different from the change produced in the blood by muſcular motion, and the conſequent increaſing deſire of a freſh ſupply of oxygen.

It has been ſaid, that this ſymptom of an alteration in the property of the circulating fluid, is one of the appearances which precede the aſthmatic paroxyſm. It is not my intention, in this place, to enquire, *how long* it may have preceded it; but it is ſufficient, for my preſent purpoſe, to diſtinguiſh it from any increaſe of frequency in the pulſations, which may ariſe *ſubſequent* to the exudation of ſerum into the lungs, and which may be occaſioned by exceſſive action of the reſpiratory muſcles.

I am

I am now to enquire what happens in the advanced ſtages of the diſeaſe.

" After ſome hours of diſtreſs," in the paroxyſm, when a depoſit of the offenſive matter has been made in the pulmonary veſicles, the frequency of the pulſe diminiſhes, and it becomes leſs full, or, as you have termed it, " more * open."

Is it not very evident, from this alteration, that the cauſe of the preceding ſymptoms muſt have been, the preſence of ſome ſtimulating material in the blood veſſels, which has now been, at leaſt partially, removed?

It is admitted, that the heart does not contract from the ſtimulus of diſtenſion only; but that its pulſations are excited by the properties of the blood. The mere increaſe of ſerum, therefore, which may have occaſioned the *fulneſs* of the pulſe, is not ſufficient to account for its celerity. No ſtate of the arteries, to which we have yet adverted, will at all illuſtrate this phenomenon. Neither hardneſs nor ſoftneſs, neither ſtrength nor weakneſs, has any thing to do with it. It is, however, ſtill poſſible to explain it, and ſome aſſiſtance may be derived, from conſidering the ſymptoms by which it is accompanied.

* Practical Enquiry, p. 72.

 " A ting-

" A tingling * and heat," in various parts of the body, is one of the moſt conſtant ſymptoms which accompany this diſeaſe; but " generally † declin-" ing as the agony of reſpiration increaſes."

This ſenſation, which I have already noticed, has been referred (but very unſatisfactorily) to a multiplicity of cauſes, and among them to "acri-" mony‡" which you have informed us, "is ‖ to be " diſcharged on the ſkin," but without explaining in what ſtate, or in what veſſels it exiſts; from what cauſes, or by what proceſs it is formed. From reaſon, however; from the ſtate of the pulſe; from the analogy of effects, which we are enabled to trace to the uſe of certain ſubſtances as part of our food; from the occurrence of the ſame ſymptom in other diſeaſes; and from its declining, at the ſame time when the frequency of the pulſe diminiſhes, namely, when the "agony of reſpiration increaſes" (when the offenſive matter has been expelled) I infer that this " tingling ſenſation and feel of heat," is occaſioned *by the acrid quality which the blood has acquired:* and I aſſume it, as a collateral proof of what has been before advanced, reſpecting the cauſe of the quick-neſs of the pulſe in Aſthma.

They who ſuppoſe that the ſenſation, above-mentioned, is occaſioned by " the ¶ difficulty with

* Practical Enquiry, p. 77.
† Ibid. 214.
‡ Ibid.
‖ Ibid.
¶ Practical Enquiry, p. 215.

" which

" which the venous blood is returned to the heart," muſt have forgotten that it *precedes* that obſtruction in the circulation; and as you, yourſelf, acknowledge, declines when the agony of reſpiration comes on.

But although theſe convincing proofs may be adduced, of the blood having acquired an increaſed power of ſtimulating the heart and arteries; there are no ſymptoms which, in the uncomplicated ſpecies of Aſthma, afford any reaſonable pretext for believing that the blood veſſels, themſelves, have ſuffered any conſiderable alteration from their uſual ſtate.

In anaſarcous habits the vaſcular ſyſtem is univerſally relaxed; the pulſe is conſequently weak, and often ſoft. In thoſe habits, alſo, of long confirmed Aſthma, which are governed by the conſtant prevalence of dyſpepſy, the veſſels become relaxed, and the pulſe is weaker, than in younger ſubjects, and in the early attacks of the diſeaſe.

In ſtudying the natural hiſtory of any morbid affection, we ſhould examine it in its ſimpleſt and leaſt complicated form; for, from thence alone, can we derive the knowledge of its cauſes.

In the firſt attack of Aſthma, the patient not being of a conſtitution greatly relaxed, the pulſe is *full*, without being remarkably weak, and *quick*, without much hardneſs. Neither of theſe ſenſations indicate that extreme relaxation of the arteries,

 which

which has been adduced, by ſome authors to account for the extravaſation of ſerum.

It is true, that a very relaxed ſtate of the veſſels does ſometimes prevail, when Aſthma takes place; but it is certainly not requiſite for the production of the diſeaſe: becauſe it frequently ariſes, and goes through its progreſs, without any ſuch relaxation being at all diſcoverable.

If the effuſion of ſerum really depended on a torpor of the capillary veſſels, ſuch a ſtate muſt neceſſarily be marked by a weak pulſe: and the ſyſtem partaking of the ſame diſpoſition,—anaſarca would as neceſſarily be a conſtant companion of Aſthma; but dyſpepſy is not *neceſſarily accompanied*, * though often followed, by watery blood.

In order to account for the excluſion of the offending matter, we have only to reſort to analogy. Every organ of the body is exactly fitted to act upon, and to be acted upon by, peculiar modifications of matter. Thus, the liver is adapted to the ſecretion of bile *from* the blood; and is ſo organized as to ſuffer an excitement, or a ſtimulus, to perform that ſecretion, by the qualities *of* the blood. Theſe qualities, being capable of great variation, the degrees of ſecretion muſt differ in proportion: and if any extraneous matter be, by any means, forced into the veſſels deſtined for ſecretion, ſuch

* Practical Enquiry, p. 240.

matter

matter will be expelled by an effort of nature, which is always aroufed to rid itfelf of noxious or oppreffive matter.

In the lefs complicated organs, as in the urinary bladder, defigned, merely, as a receptacle for the aqueous part of the blood, feparated in the kidneys; extraneous bodies (fuch as calculi) are no fooner formed, than the containing parts are ftimulated to expel them. Whether the effort produced, be fufficient for that purpofe or not, makes no part of the prefent enquiry.

Urine may be accumulated in a very confiderable quantity, before it excites the bladder to contract, and expel it, by the ftimulus of diftenfion only; but when the contained fluid has acquired an augmentation of its ftimulating property, the bladder may contract, on a very fmall quantity, with great violence. In like manner, the gall bladder, the ftomach, the inteftines, and, in fhort, every containing part, is irritated to endeavour at the expulfion of that which offends it, or has a tendency to interrupt its * functions.

The effort of the heart and arteries is thus excited, by the acrimony of the ferum circulating through them in Afthma; and their extremities, partaking of the irritation, open their orifices, and exclude the offending matter.

* Practical Enquiry, p. 16.

The

The circulation is then reſtored to a more natural ſtate, and remains ſo, until an increaſe of acrimony again ſtimulates the veſſels, and reinduces the paroxyſm.

This accounts for the diminution of frequency in the pulſe, when the extravaſation of ſerum has been accompliſhed; and explains the phenomenon of irregular periodical returns of the diſeaſe, without allowing to the capillaries "a power * of contracting, independent of the trunks from which "they proceed;" or aſcribing to them "a debility† "which does not, proportionably, correſpond with "the tone of the larger veſſels." It points out the true cauſe of the aſthmatic paroxyſm, in thoſe caſes, in which *you* have attributed it to the injudicious ‡ uſe of the lancet: or to acrimony, which, in another place, you have ſaid, is "always pre-"ſent *in the atmoſphere*, and ready to be ‖ inſpired." It deſtroys the ridiculous idea of Aſthma, having been induced by the uſe of mercurial preparations**, and alſo ſatisfactorily explains thoſe ſymptoms which have often been referred to various, and, indeed, oppoſite †† cauſes; it ſuperſedes the neceſ-

* Practical Enquiry, p. 158.

† Ibid.

‡ Ibid. 206.

‖ Ibid. 318.

** Chamberlaine's Treatiſe, &c. p. 10.

†† The notion of ſympathy, which Heiſter, not improperly, termed, an ENTHUSIASTIC DOCTRINE *, has alſo furniſhed the

* Heiſter's Surgery, Part 2d. Sect. 1. C. 2.

ſhadow

ſity of Dr. *Girtanner's* hypotheſis reſpecting the inſenſibility of the heart, which he has *luckily* not *attempted* to prove, becauſe he could *not have proved* it, if he had attempted it: and it elucidates the cauſes of an intermitting pulſe, without the doctrine of "*the heart ſenſibly* * *feeling the want of* "*ſtimulus*;" which reminds me of Mr. *John Hunter*'s obſervation, who was accuſtomed to ſpeak of *the ſtimulus of death:* an expreſſion, perhaps, the leaſt determinate of any of which he could have made uſe; for it muſt mean a ſenſation of which no man can form the leaſt idea, *who has not* felt it, and which no man can deſcribe, *who has.*

But if it could be conceived poſſible, that a partial atony of the capillary veſſels might prevail;

ſhadow of a cauſe of Aſthma; which, though ONLY A SHADOW, has been eagerly caught at, by the older writers. You remark, that "WILLIS ſpeaks of Aſthma from SYMPATHY, ariſing from A "STONE in the gall bladder." (page 331.) The caſe alluded to, was, I ſuppoſe, that which is introduced in his "Pathology of the "Brain and Nerves"; of a "Butcher at Walſall," (which, by the bye, is given at SECOND, or perhaps, third hand, for WILLIS obſerves, that "it was communicated" to him, "by the learned Dr. WALTER "NEEDHAM," who happened to reſide at the CHARTER-HOUSE, in LONDON:) about whoſe ſymptoms we are entirely in the dark, though not more than WILLIS, himſelf, who has not attempted to deſcribe them; and, therefore, his idea of the man's diſeaſe ariſing in conſequence of biliary obſtruction, or to tranſlate his words more literally, of "the gall bladder containing many ſtones," might have been deemed capricious and unſatisfactory; even if NO BETTER theory could be ſuggeſted.

* Practical Enquiry, p. 235.

the

the exhalents of the lungs only, being affected, it would then become a queſtion, how is the tone of theſe veſſels reſtored?

After a certain period, we find, that the paroxyſm of Aſthma terminates, ſpontaneouſly, by the expectoration of a larger or ſmaller quantity of mucus; but no freſh depoſit is made for ſome time afterwards. If the extravaſation took place in conſequence of a relaxed ſtate of the arterial extremities, become incapable of contracting with their accuſtomed energy; theſe veſſels would continue to permit the exit of their contents ſo long as any ſerum remained of a thin conſiſtence, or until they had regained their contractile power.

When the diſeaſe is not left entirely to itſelf, the remedies exhibited during the paroxyſm, though of various kinds, and very diſſimilar in their modes of operation, are in no wiſe of ſuch a claſs as to afford the leaſt probability that they have, at any time, contributed to reſtore the energy of the exhalent arteries, or to invigorate the ſyſtem.

Hence it appears, that the effuſion of ſerum, in Aſthma, is *not* neceſſarily to be referred to a preternatural relaxation of the blood veſſels: notwithſtanding all the arguments which have been waſted in ſupport of that hypotheſis.

I have already offered, what I conceive will be admitted as ſufficient proof, of the exiſtence of an

increaſe

increaſe of ſtimulating power, in the blood of perſons afflicted with Aſthma.

The next ſtep is, to aſcertain in what manner ſuch an alteration of quality be acquired: and this enquiry I ſhall endeavour to purſue by the ſame plain method of argument, which, I truſt, has been ſucceſsfully employed in the preceding pages; not forgetting Sir Iſaac Newton's remark, which you have quoted, "Fruſtra fit per pleura, quod "fieri poteſt per pauciora."

The appearance of blood, when examined out of the body, in, apparently, the ſame circumſtances, is found to vary very conſiderably: for the atmoſpheric air, being capable of effecting a change in its colour, and the temperature of the ſurrounding medium, either accelerating or retarding its coagulation, and the ſhape of the veſſel into which it is received, and the ſize of the ſtream by which it flows, having each its reſpective ſhare of influence upon it; there is a conſiderable degree of difficulty in conducting experiments on this important fluid, with ſufficient accuracy.

The component parts of the blood have, however, at length, been aſcertained. The firſt, and moſt obvious diviſion is into red particles*, coagu-

* FORDYCE.

See, alſo, the works of BOYLE, SENAC, HALLER, JURIN Father de la TORRE, HUNTER, but particularly HEWSON's Experiments.

lable lymph, and ſerum. It is unneceſſary, on the preſent occaſion, to purſue the examination beyond this view of the matter: becauſe, if it be admitted that either of theſe parts, being increaſed or diminiſhed in quantity, muſt create an alteration in the whole maſs, it will be readily conceived that each of them, being in itſelf a compound, will neceſſarily vary, in proportion to any change which may have been effected in the parts of which it is compounded.

In ſome diſeaſes, the texture of the blood is broken down to an infinite minuteneſs; in others, the whole maſs is rendered glutinous, by the deficiency of ſerum: in others, a vaſt quantity of ſuperfluous water is circulated in the veſſels, in an uncombined ſtate.

By the want of one of the properties, uſually ſuperadded to the blood, by the atmoſpheric air in the lungs, its colour becomes altered. In ſuch caſes it may be conſidered, either as merely deficient of a neceſſary component part, which ought to have been communicated to it; or as having retained ſome excrementitious particles, which that proceſs would have removed.

Beſides theſe diſorders and alterations in the blood, it is liable to be mixed with various fluids, ſecreted by different organs of the body. For example, in jaundice, a very copious ſecretion of bile

bile takes place, which is conveyed into the circulation by the abſorbents.

Bile is a fluid, whoſe colour, taſte, and ſmell, are ſo well known, that no perſon, who has ever ſeen the blood of a patient in jaundice, can poſſibly miſtake its appearances, or refer them to any other than their true cauſe.

This ſingle fact is ſufficient to prove, that *the abſorbents may convey into the blood veſſels, a fluid capable of effecting an alteration in the taſte, colour, and qualities of the ſerum.*

Experience, alſo, convinces us, that the finer parts of Cantharides, applied to the ſkin, are abſorbed into the blood veſſels, and paſſing from thence to the bladder, irritate it by *a conſtant ſtimulus*, to attempt the evacuation of its contents: an effect never produced by the urine in a healthy ſtate.

The odorous parts of aſparagus, and of garlick, are alſo found to combine very readily with the blood, and to paſs off with the urine.

Almoſt all the authors, who have treated of the doctrine of Aſthma, have agreed in aſſigning to it, dyſpepſy as one of the prediſponent cauſes, which "induces that *ſtate of the body* in which the "former diſeaſe takes place."

In this, you, Sir, have concurred with the great and the learned, the ancient and the modern writers, to whoſe works you have ſo frequently referred; but, like them, have gone no further, nor informed your readers what that ſtate of the body is: nor *how* * dyſpepſy does produce it. "Wherever," you remark, "dyſpepſia prevails, there ſhall we "find a *fruitful* opportunity of exciting the pa- "roxyſm of Aſthma; but this morbid debility of "the ſtomach *muſt probably concur with accidental* "*cauſes*, before the diſeaſe appears†."

The laborious induſtry and ingenious experiments of the Abbé Spallanzani, Dr. George Fordyce, and others, have proved in what manner chyle for the ſupport of animal life, by its converſion into blood, is ſeparated from the food by the proceſs of digeſtion; and it has been ſhewn that if any ſubſtances, capable of an alteration, in any degree of temperature, not greater than that of the body, ſhould be retained in the ſtomach, without being digeſted; or, in other words, if the ſtomach, by

* It is really aſtoniſhing that authors ſhould have approached ſo near to the truth, without perceiving it. Dyſpepſy has been obſerved to ACCOMPANY Aſthma, and yet the manner in which the latter was influenced by it, has been totally overlooked; and even the moſt obvious mode, by which the remedies employed, were productive of advantage, entirely neglected. Some of the miſchiefs of Indigeſtion were noticed, at a very early period; even Hippocrates was aware of them, and Galen attributed * ALL diſeaſes to that ſource.

* Galen, lib. i. de cibo.

† Practical Enquiry, p. 260.

diſeaſe,

diſeaſe, or by a deficiency of any of the materials neceſſary to be employed in digeſtion, ſhould have become incapable of performing that proceſs, even for the ſhorteſt ſpace of time, while ſubſtances which might otherwiſe be converted into chyle, remain in it; a fermentation takes place, by which an acid is *neceſſarily* diſengaged:—" the formation " of acid * being always produced by the digeſtion " not going on perfectly."

Acid, therefore, may be formed in very conſiderable quantity; and it may be ejected through the inteſtines, in conſequence of the irritation which is produced by it, or by the proceſs which takes place at its formation. But it may alſo be retained in the ſtomach and inteſtines, till the abſorbents, being ſtimulated, convey it with the chyle into the blood veſſels.

Among all thoſe circumſtances which are ſaid to produce a prediſpoſition to Aſthma, Dyſpepſy ſtands foremoſt. " Dyſpepſia is a condition of the " habit which will be found *always* to have preced- " ed the periodic Aſthma†." Permanent Dyſpepſy is that ſtate in which a great quantity of acid is extricated from the undigeſted aliment;——the acid thus formed, I conſider, therefore, to be the *real cauſe* of Aſthma; and it is certainly no ob-

* Fordyce on the Digeſtion of Food.
See alſo Spallanzani on Digeſtion.

† Practical Enquiry, p. 217.

jection

jection to this opinion, that it may be urged Dyſpepſy does not *always* produce that diſeaſe.

It is undoubtedly neceſſary that the acid be formed in conſiderable quantity, and that it acquire a certain degree of bulk (if I may be allowed the expreſſion) before its ſtimulating power can be exerted, with ſucceſs, in the blood veſſels. I have ſhewn that the pulſe is not without an indication of the ſtimulus of diſtenſion, as well as of quality; and my obſervations are ſupported by your own remark, that quality and quantity may combine their efforts.

It has been obſerved that the diabetes, which frequently accompanies Aſthma, "does * not, in "fact, apparently alleviate the labour of reſpira-"tion; it ſeems, on the contrary, to *precede*, or *ac-"company*, the effuſion into the veſiculæ, inſtead of "following it:"—it is, therefore, undoubtedly dependent on the ſame cauſe which produces the effuſion into the lungs,—the irritation of acid ſerum in the blood veſſels.

If, then, a definition of Aſthma be required, I have no objection to call it, *an exceſſive contraction of the reſpiratory muſcles, excited by the irritation of acid ſerum effuſed from the pulmonary veſſels into the veſiculæ and bronchia.*

* Practical Enquiry, p. 228.

It

It may, perhaps, be aſked, How comes it to paſs that the uſe of *vinegar* in the paroxyſm of Aſthma is productive of ſo much benefit, if the diſeaſe really depend on the predominance of acidity?

To which I anſwer. This remedy is confined to the paroxyſm; in which it is preſcribed as an expectorant,* and, perhaps, a diuretic, and no practitioner would think of employing it, *afterwards*, as a tonic; or, for the purpoſe of reſtoring thoſe powers of the ſyſtem, whoſe healthy action is to prevent the recurrence of Aſthma.

Thus, emetics are advantageouſly preſcribed, for the purpoſe of removing offenſive matter, ſuch as indigeſtible or fermenting aliment, from the ſtomach; but no one would think of uſing them to

* The ancients ſeem to have conſidered ALL acids as ſimilar in their mode of action:—thus the muriatic acid, or, as it was formerly called, "ſpirit of ſea ſalt," was greatly praiſed by Van-Helmont "EST "NAMQUE ACIDISSIMUS" (ſaid he) "nec ſibi par habet remedium, "extinguendis ardoribus urinæ etiam præſtanti in veſica calculo."

Vinegar is alſo a very old remedy, having been long celebrated as a powerful diuretic and anti-emetic. It was ſtrongly recommended by Dr. BAYNARD, Dr. BENTLEY, Mr. KEMP*, Dr. COLBATCH†, and other practitioners of the laſt century, who only improved the compliments beſtowed on it, by CELSUS, DIOSCORIDES and MINDERERUS:—it was preſcribed by BOERHAAVE as a ſudorific, and has ſince been noticed by DARWIN AS AN ABSORBENT, in terms nearly ſimilar to thoſe adopted in the "Practical Enquiry."

* Author of "a Treatiſe on the Peſtilence."

† Author of "Phyſico-medical Eſſays: Novum lumen Chirurgiæ, &c.

invigorate

invigorate the body, or to increaſe the permanent ſtrength and tone of the ſtomach, afterwards.

Throughout the whole courſe of this inveſtigation, I have hitherto conſidered the Theory which I have endeavoured to eſtabliſh, as deſtitute of any other proof than its own reaſonableneſs, and the evident relation between cauſe and effect: but I have not yet put forth *half my ſtrength*, and as it may afford ſome degree of ſatisfaction to thoſe who are deſirous of ſeeing juſt theory founded on its moſt certain baſis, I think it right to add, that the opinion which I have ventured to deliver, is ſupported by a ſeries of experiments, conducted with equal accuracy and judgment, by Dr. Bache, Phyſician at Birmingham. This gentleman, whoſe diligent and ſcientific attention to every ſubject of practical importance in medicine is only equalled by his candour and integrity; was induced to inveſtigate the different ſecretions of an aſthmatic patient, ſo long ago as about the year 1784.

In this caſe, which is very diſtinctly related, "the * ſaliva was remarkably frothy, and the expectoration extremely viſcid. The ſurface of the urine (after it had ſtood a few hours) ſeemed as if covered with duſt," which, being collected and dried, was "ſubjected to examination by the microſcope," and proved to be ſaline cryſtals," but a

* Medical and Phyſical Journal, Vol. 2, p. 140.

ſufficient

ſufficient quantity could not be obtained, for any chemical experiment.

Lime-water was precipitated by this urine with greater celerity, than by the urine of a perſon in health.

" The * matter perſpired, ſmelt *ſour*; and when " pieces of bibulous paper were previouſly ſtained " by a ſolution of litmus, and then applied to " various parts of the body, the exudation from " each, produced the effect of a weak acid upon " the colour; and when the paper was made dry, " and the edge of it applied to the flame of a " candle," it was " found to be a weak touch pa- " per; but when immerſed in a ſmall glaſs of water, " in which a grain or two of vegetable alkali had " been previouſly diſſolved, and dried a ſecond " time, its property as a touch paper was greatly " augmented."

In ſhort, the reſult of the Doctor's enquiry convinced him, that " *an* † *acid pervaded the whole of* " *the circulating ſyſtem:*" and ſuggeſted the mode of cure, which was adopted with complete ſucceſs.

It is with equal reſpect and gratitude that I embrace this opportunity of acknowledging my

* Medical and Phyſical Journal, Vol. 2. p. 141.
† Ibid.

obligations to Dr. Bache, for the alacrity with which he politely communicated to me the particulars of the experiments, above alluded to, and the very liberal and candid manner in which he favoured me with many valuable remarks on the ſubject.

The intereſts of ſcience, are cloſely connected with the ingenuouſneſs and liberality of thoſe who walk in her paths; and I am perſuaded that every one of my readers will concur with me, in the opinion, that he who imparts to his friends, without reſerve, and communicates to the world, without oſtentation, the reſult of judiciouſly directed ſtudies, and attentive experience, adds luſtre to his character as a Man, by his conduct as a Practitioner.

To proceed;—ſome of the ſecretions have been examined by others, from different motives, and with different views. The copious evacuation of urine which frequently takes place, at the time when the ſerous extravaſation, into the veſicles commences, has occaſioned that fluid to be more particularly attended to. I do not know whether its *acidity* has been often obſerved, nor (if detected) that it has always been remembered: but, although it is poſſible that the urine may not *invariably* partake, in ſo great a degree, of the prevailing acidity; this, by no means, diſproves the general theory which I am anxious to eſtabliſh;—but only ſo far anxious, as it will lead to a more certain mode of

practice;

practice; and encourage the faculty to regard what has hitherto been considered, only an accidental concomitant of the disease, as the *primum mobile.* You, Sir, have examined this secretion, and, certainly, do not mention its having acquired the property I have described; but although I am far from doubting the accuracy of your statement, and am not inclined to dispute your *taste**, I humbly conceive that such an alteration might have escaped detection, in the mode you observed; particularly as the flavour of urine cannot be supposed very familiar to the palate.

If you ask in what manner acids are productive of irritation, I will candidly confess, that I am entirely ignorant of it.—It appears to me, sufficient, that I have proved the formation of acid, its con-

* "If the Urine is copious and pale, it is, I believe, never "SWEET." Who ever supposed that it was?

"I have TASTED the water of an Asthmatic more than once, "when the circumstance of quantity and appearance strongly solicited "my curiosity, but I always found it WEAK, SALINE, and of no "SACHARINE TASTE whatever." Practical Enquiry, p. 248.

The judgment which some have pretended to form of the nature and symptoms of diseases, from an examination of Urine, by THE EYE AND THE PALATE, has been often exposed to deserved ridicule; and as no real dependence can ever be placed on SUCH observations, it is unfortunate, as well as surprising, that practitioners have not been deterred from recording them, by the example of a late celebrated Physician at BIRMINGHAM, who is said to have gravely decided on a state of pregnancy, from the inspection of Urine, which, in reality, belonged to a little girl. It is, I suppose, unnecessary to add, that the learned Doctor immediately received his fee of dismission: and that the respect and confidence of his patient was not GREATLY INCREASED by this proof of physical sagacity.

veyance into the blood veſſels, and the ſubſequent effects which are produced by it. I pretend to no further knowledge of the manner in which theſe events ariſe: and it would be far more conducive to the true intereſts of humanity, and the advancement of ſcience, if men would more frequently content themſelves with attentively obſerving the appearances of effects*, and faithfully recording them, rather than ſeek after Fame in the wide fields of Theory and Hypotheſis†. We ſubject ourſelves to the reproaches of our contemporaries and the ridicule of poſterity, by vain efforts to explain what exceeds our comprehenſion; and often become, like the blind man (mentioned by Mr. *Locke*‡,) who attempting to deſcribe a ſcarlet *colour*, ſaid that it reſembled *the ſound of a trumpet.*

In almoſt every diſeaſe, there are ſymptoms for which human ſagacity has not yet been able ſatisfactorily to account. Let us exert our unremitted endeavours to arrive at a more correct knowledge of them; but be always more ready to confeſs our ignorance, than to diſguiſe it by affectation; for "the ‖ human mind has its horizon as well as the "organs of viſion. The boundaries of nature are "extended beyond our reach; and as the laws of

* Sydenham.

"† It becomes the Phyſician to repreſs thoſe wild ſallies of imagi-"nation which lead to ridiculous hypotheſes, and to admit only ſuch "facts as are eſtabliſhed by accurate obſervation."

Fourcroy's Elements of Chemiſtry, Vol. 1.

‡ Locke on the Human Underſtanding.

‖ Bache on Electricity.

"matter

" matter are too much divaricated for its complete " inveſtigation, it becomes us to remain ſatisfied, " if aſſiduity, and the exertion of our mental powers, " can bring us within the ſphere of probability, " where mathematical demonſtration, or *poſitive* " *proof*, cannot be obtained."

It is poſſible that *the nature* of the *acid*, itſelf, which I conſider as the *cauſe* of Aſthma, may differ in particular caſes: nor do I altogether deſpair of being able to purſue this enquiry with conſiderable advantage, if many opportunities of attending to the diſeaſe ſhould be afforded me.

It remains for me to ſay a few words on the ſubject of medical treatment.

There are ſo few medicines, which, being conſidered *a priori*, would appear capable of relieving diſeaſes, that it is not ſurpriſing that many of the moſt valuable ſhould have been the diſcovery of accident: and the almoſt inſuperable difficulty of explaining the manner in which they produce their effects, has occaſioned Practitioners, in general, to continue their uſe, without much reflection.

Ingenious men, excited by curioſity, and encouraged by the activity of their imagination, have ſometimes indulged themſelves in ſpeculating on this intricate ſubject; but the degree of certainty, to which the utmoſt efforts of human intellect have been able to attain, has hitherto been productive of,

comparatively,

comparatively, few improvements in the practice of medicine.

GOD has been pleaſed, in his wiſdom, to eſtabliſh a relationſhip between the affinities of bodies, by a ſeries of dependencies, which, ſpringing one out of another, conſtitute an univerſal chain; of whoſe links, only a few are perceptible by the limited powers of his creatures. Theſe links then, are called cauſes and effects, but our imperfect diſcernment, only graſps at ſome of them, continues its reſearches through a few more, then loſes itſelf in viſionary hypotheſis or vague conjecture; jumps, perhaps, again, upon a different part of the ſeries, and ultimately acquires juſt ſufficient knowledge of the whole, to admire the wonderful intelligence and perfection of its primeval cauſe.

But, notwithſtanding this ſtate of imperfection, the mind is endowed with a conſtant activity, never reſting ſatisfied with its preſent acquirements, but delighting in purſuing its ſpeculations, although ſo unfortunately prone to wander from the truth.

It is only by attending to the experience that one event * ſprings out of another, that we eſtabliſh the idea of the mutual relation between cauſe and effect: and the mind being capable of ſuppoſing an exiſtence, *independent* of any cauſe, is

* Fordyce.

apt

apt to loſe ſight of the *dependence* which other objects have on one another; and to ſubſtitute an imaginary connexion inſtead of that which cannot, perhaps, be traced without great difficulty, and the moſt attentive inveſtigation.

Hence have ariſen that vaſt variety of fanciful theories, which, being founded on error, are left to float at random, without ſtability or advantage. In phyſic, as in other ſciences, "error and obſti-"nacy, the pride of ſyſtem, and the influence of "authority, have conſpired in too many inſtances, "to obſcure the plaineſt indications of nature*:" and we ought to think ourſelves happy, if, by patient induſtry, we can obtain the poſſeſſion of two or three links, at no great diſtance from each other, in the chain of cauſes and events; for, although the part which connected them, may, perhaps, be irrecoverably loſt, the mechaniſm of thoſe links which we have diſcovered, will, ſometimes, lead to a tolerably concluſive idea of its ſhape and texture.

Thus we know that a grain of wheat, being put into the ground, produces a plant, which, when it has arrived at maturity, exactly reſembles that from which the ſeed was taken:—repeated experience convinces us of the fact, although no human ſtudy nor exertions have been able to diſcover *how* the germination of the plant, or the evolution of its parts is effected.

* Practical Enquiry, p. 322.

Proofs

Proofs have been adduced in the preceding pages, that a great quantity of acid being extricated in the fermentation of undigeſted aliment, is conveyed into the blood veſſels, and ſtimulates them to expel it: but I know no more *how* or *why* it is the *nature of this acid* to produce ſuch an effect, than I know *why* grain buried in the earth ſhould produce a plant.

In the very nature of things it is neceſſary that matter, ſhould, at its original formation, be endowed with *diſtinct properties*; and it is ſufficient *for me* to believe, that the all powerful Creator has diſtributed theſe properties with infinite wiſdom: and this belief reſults from analogy, from contemplating that beautiful harmony of arrangement, and excellence of contrivance, manifeſted in thoſe things which my ſenſes *can* comprehend.

Leaving, therefore, all metaphyſical reaſoning on the *modus operandi*, I proceed to notice ſuch medicines as have been generally reſorted to, for the removal of the paroxyſm of Aſthma, or to prevent its recurrence.

If any proof be required of the erroneous notions, formerly entertained, reſpecting this diſeaſe, it may be obtained from a review of the medicines which have been employed for its removal.

In

In the number of thoſe preſcribed for aſthmatic patients, many, very contrary in their tendency and effects, have, at different periods, obtained the favourable opinion of practitioners; who have exerted no inconſiderable degree of ingenuity, in varying their different forms * and combinations.

The ancients, ſuppoſing that *a defluxion from the head* was the cauſe of Aſthma, maintained the neceſſity of uſing evacuants. The moderns, as if to complete the perfection of former error, have ſometimes † employed them at the commencement of the paroxyſm: but, as a mere removal of undigeſted aliment, is the only real benefit which they are capable of affording; cathartics ſhould only be uſed as aſſiſtants in the general intention of reſtoring the healthy functions of the organs of digeſtion.

Emetics, having ſome tendency to promote expectoration, and likewiſe diminiſhing the quantity of acid formed in the ſtomach of an Aſthmatic, were found ſerviceable; although they were undoubtedly not employed with the latter intention, either by Baglivi, Etmuller, or Sir John Floyer: but every violent effort of the ſyſtem, at the beginning of the paroxyſm, muſt evidently be injurious.

* Withering.

† Practical Enquiry, p. 360.

Long before the extravaſated ſerum was conſidered to be the exciting cauſe of the convulſive efforts of the muſcles of reſpiration, *expectorants** had been tried, and their effects found highly advantageous. They have been conſtantly and deſervedly retained, from their firſt introduction into practice, to the preſent time.

Beneficial effects have been aſcribed to bliſters, but as the mode by which they produced them, was not at all underſtood; it has likewiſe been denied † that they were the means of relief.

The prevalence of acid irritating the blood veſſels being proved, the uſe of bliſters is at once explained, without ſuppoſing them capable of leſſening the diſtenſion ‡ of the ſtomach.

As to their promoting ‖ expectoration, which muſt depend on their excitement of muſcular action; it would be juſt as reaſonable to ſuppoſe that

* The bitter Vetch and Naſturtium were recommended as expectorants in diſeaſes of the breaſt and lungs, at a very early period. The former is mentioned by PLINY, as having cured AUGUSTUS CÆSAR of a diſeaſe which was ſuppoſed to have ariſen from viſcid phlegm in the lungs. Even Vinegar of ſquills is recorded in one of the books which bear the name of GALEN.

See Parkinſon's Theatrum Botanicum, p. 1079, London Edition, 1640.

† Practical Enquiry, p. 374.

‡ Ibid. 375.

‖ Ibid.

the

the ſpeed of a horſe would be increaſed, or the ſtrength of a gold-beater's arms, by applying a bliſter to his back.

It is unneceſſary to expatiate on the inutility of fumigations of all kinds; nor do I think it worth while to combat the *viſionary* idea of *aerial* medicines being capable of ſubduing the violence of the paroxyſm, *before* the ſerous effuſion has been expectorated.

After commenting upon the uſe of Vinegar in the paroxyſm, you, Sir, have been pleaſed to expreſs "great hopes of nitre in combination" * with that acid. As I do not very well underſtand the rationale of ſuch a preſcription, I will only obſerve, that this mixture, having been on record † for five and twenty hundred years, it is to be lamented that no determinate ideas of its efficacy have been hitherto eſtabliſhed.

I hope it will not be expected that I ſhould be particular on the effects of two remedies ſo directly oppoſite, as hot ‡ and cold bathing. The latter, as

* Practical Enquiry, p. 384.

† Bibl. Sacr. An. A. C. circa 700.

‡ "Not contented with probability, R. B." (Dr. BREE) "went "into the hot bath, both in the intermiſſion and the paroxyſm. The "reſpiratory diſtreſs was much aggravated," &c.

"In the paroxyſm R. B. went into a bath of forty-ſix degrees. "The great abſtraction of heat was evidently injurious, and it was "ſome hours before the impreſſion was ſo far overcome, as to take "away fears of the conſequence." Practical Enquiry, p. 389.

This might not unaptly be termed ARGUMENTUM AD HOMINEM!

a tonic, requires no new panegyric:—the former appears ſo directly repugnant to all the reaſoning which I have been able to apply to the ſubject, that I have never thought of directing it.

When the theory of ſpaſm was *faſhionable*, thoſe practitioners who embraced it (and a vaſt number of pupils who ſuppoſed it neceſſary, that they who had been brought up at the feet of Dr. *Cullen*, ſhould defend *all his whimſies*, *did* embrace it) thought themſelves ſuddenly in poſſeſſion of the philoſopher's ſtone. Every diſeaſe was referred to it; all former doctrines were ſacrificed at the altar of the idol, and its influence was extended over every medicine preſcribed by its devotees.

Antiſpaſmodics were tried in Aſthma, and tried in vain. Zinc*, caſtor, muſk, camphor, opium, and, in ſhort, every preparation, with which the ingenuity of modern pharmacopoliſts has furniſhed us, have been repeated, and unſucceſsfully repeated. Apo-

* Zinc ſcarcely deſerves to be enrolled in this liſt, notwithſtanding "the opinion of Gaubius*," for although it has been recommended in Epilepſy, and employed, (it is ſaid) ſucceſsfully in † Germany, though unſucceſsfully in ‡ Great Britain; it is not very clear that it poſſeſſes any antiſpaſmodic quality whatever. If it does, it is ſcarcely neceſſary, now, to obſerve, that it is not likely to be productive of benefit in Aſthma.

* Practical Enquiry, p. 368.

† Fourcroy's Chemiſtry, Vol. 2. p. 391.

‡ Pharmacopœia Col. Reg. Med. Lond. [Engliſh Edition] p. 187.

plectic

plectic ſtupor*, the dreadful foe of rational exiſtence, has been boldly dared, with obſtinate and reiterated

* "In the acceſs of a paroxyſm R. B. took FOUR grains of ſolid "opium, which produced, nearly, an apoplectic ſtupor for two days; "a countenance more turgid than uſual, and intenſe head ache attend-"ed. The paroxyſm ſhewed itſelf four hours earlier, than uſual, the "next day, and TWO grains more were taken when it was perceived "to commence:—anxiety encreaſed to an alarming degree as the "ſtupor became ſomething leſs. Looſe motions ſucceeded, and a gene-"ral ſweat. A medical friend became alarmed." &c. Bliſters were ap-plied; Vinegar, and Pepper, and Muſtard, and Coffee were given, and "the patient was at laſt brought back to a ſtate more uſual in former "paroxyſms, but with every care the exacerbations were no fewer than "nine" before the end of the fit. But, "notwithſtanding the bad "ſucceſs of this experiment. Opium gr, ij was uſed after an active "vomit, and bad conſequences ſtill enſued:" ſo that, to uſe Dr. BREE's words on another occaſion, "this gigantic remedy was nei-"ther productive of gigantic fame nor eſtimable reputation." p. 372.

The above reminds me of a ſtill more concluſive experiment of the late eccentric Dr. STARK; and as his opinion has been reſorted to, in the "Practical Enquiry" (page 175) this will, I truſt, be a ſufficient apology for introducing the following anecdote.

Dr. STARK * was employed ſeveral months in making experiments on his own frame, of the effects of different ſorts of food, confining himſelf to a ſingle kind for ſeveral days. He ſubſiſted for ten days, on honey and bread, and a pudding compoſed of flour, water, and honey, by which a diarrhœa was produced, for which he ate a large quantity of Cheſhire cheeſe daily, without uſing any other food or medicine. The diarrhœa was completely ſtopped. He then cauſed himſelf to be blooded; took a mixture of Tartarized Antimony and Rochelle Salts, which reinduced the diarrhœa,—with great debility, and anxiety about the præcordia: he became delirious, and died, at the age of twenty-nine, a Martyr to his own imprudence, and an Example to all future EXPERIMENTALISTS.

Dr. STARK has frequently been applauded for his induſtry and perſeverance, but when we reflect on their fatal conſequence, we ſhall be compelled to admit, that although he had ZEAL, it was "not ac-"cording to KNOWLEDGE."

* See Monthly Review for April, 1790.

temerity.

temerity. Large and repeated doſes of opium have been taken, in oppoſition to juſt reaſoning, and in defiance of diſtreſsful experience.

Of ſtimulants I have nothing to ſay; and ſhall cloſe my remarks on the medicines in common uſe, with obſerving, that a *proper idea* of the nature and cauſe of Aſthma, being once eſtabliſhed in the minds of practitioners,—the indications for its removal and prevention will become extremely obvious.

Firſt, to remove the inſtrument of offence: and ſecondly, to prevent its regeneration.

The firſt, compriſes the remedies which are required in the paroxyſm.

The ſecond, thoſe which being uſed after the termination of the paroxyſm, have a tendency to prevent its recurrence.

Expectorants may be advantageouſly employed, to remove the ſerous effuſion, oppreſſing the veſiculæ and bronchia: and as the blood partakes of the cauſe of offence; whenever conſiderable fulneſs of the veſſels be indicated by the pulſe, at *the commencement* of a paroxyſm, blood letting may be directed with a fair proſpect of relief; and bliſters may be ſucceſsfully applied.

When

When the violence of the paroxyſm has been ſubdued; whatever has a diſpoſition to correct acidity, and to reſtore the tone of the ſtomach, and other organs of digeſtion, is expreſsly indicated.

The benefit which has been derived from the uſe of *chalk*, during the prevalence of acidity, is too obvious to be overlooked entirely, or to demand my particular notice. But in the character of an *abſorbent* only, can the advantages of chalk to aſthmatic patients, be rationally accounted for: and your *concluſion* "that the capillary orifices of "the ſtomach demand ſome appropriate aſtringent "to excite their contractions," affords no real elucidation of the manner in which the uſe of *chalk* produces benefit. Quincy, * indeed, ſpeaks very poſitively of the great aſtringency of chalk, but his authority is oppoſed by that of the indefatigable Dr. Lewis, † as well as many other reſpectable writers on the Materia Medica.

Bitters, Steel, and all that claſs of medicines which has been denominated corroborants, may be advantageouſly reſorted to.

Chalybeate waters, which have a tendency to obviate coſtiveneſs, have been long eſteemed as highly beneficial.

* Quincy's Pharmacopœia officinalis, p. 108.
The ſame author talks of aceſcent vegetables "chilling the juices, "and occaſioning them to run into corruption and putrefaction"!

† Lewis's Diſpenſatory, p. 130, art. Creta.

Every

Every habit of life, which, in any degree, affects the ſtate of the digeſtive organs, ſhould be carefully attended to.

The mere influence of habit, or ſympathy, can only diſpoſe a perſon to be more readily affected by diſeaſe: but habitual modes of life which tend to diminiſh the tone of the ſolids, ſhould be avoided: and although it may ſeldom be practicable (if it were thought ever ſo highly advantageous) for an aſthmatic to change his reſidence and alter his mode of life, and the nature of his purſuits entirely; a relaxation from the fatigue of ſerious ſtudy, and the dyſpeptic provocation of ſedentary employments ſhould, if poſſible, be inſiſted upon.

Exerciſe on horſeback is of ſingular advantage; and a careful attention to the ſpecies of * clothing worn by dyſpeptic patients, is of great conſequence.

In every particular caſe, the fit and the appropriate, muſt be left to the judgment of the practi-

* "The perſpiration of the human body, being abſorbed by a "covering of flannel, is immediately diſtributed through the whole "thickneſs of that ſubſtance, and by theſe means expoſed, by a very "large ſurface, to be carried off by the atmoſphere; and the loſs of "this watery vapour, which the flannel ſuſtains on the one ſide, by "evaporation, being immediately reſtored from the other, in conſe-"quence of the ſtrong attraction between the flannel and this vapour, the "pores of the ſkin are diſincumbered, and are continually ſurround-"ed by a dry, warm and ſalubrious atmoſphere."

Sir Benjamin Thompſon's (Count Rumford) "Experiments" inſerted in the "Repertory of Arts," No. xxii, p. 251.

tioner,

tioner, and after thus explaining the *general intentions*, it would be not only uſeleſs, but impertinent, to attempt entering into a more minute view of the remedies which ought to be employed; for, notwithſtanding whatever has been, or may be advanced, by the unlearned or the *credulous*, reſpecting *ſpecifics*; in medicine there is no ſuch thing * as an univerſal law.

That modification of the diſeaſe which was formerly denominated dry or nervous Aſthma; but has ſince been diſtinguiſhed by the term *convulſive*; (and which conſiſts of periodical returns of paroxyſms, in which the action of the reſpiratory muſcles, is, perhaps, equally violent, as when they contract, in conſequence of a large quantity of mucus to be expelled from the lungs;) is a very rare occurrence, when the conſtitution has been once reſtored to its accuſtomed energy.

The diviſibility † of matter is ſo incalculable, that it may be conceived poſſible for a ſpecies of irritation to prevail, of a ſimilar kind to that which occaſions the paroxyſm of Aſthma; in conſequence

* Profeſſor Storck's Works.

† I will acknowledge that I was much amuſed with your remark on this ſubject; when ſpeaking of "the quantity of air extricated from "pounded apples," which, you obſerve, from Dr. Hales, "give out forty eight times their own bulk;" and immediately add, "this air muſt have been condenſed into leſs than a forty eighth part of "the ſpace it takes up when free from them" !!

Practical Enquiry, p. 267.

of a ſtimulus, of which the particles are too minute, even when in their groſſeſt ſtate, to come within the ſphere of our obſervation.

It is in theſe caſes that the uſe of anti-ſpaſmodic medicines is found to be of ſervice: although the particular mode of their operation is very imperfectly underſtood.

It has been ingeniouſly ſuggeſted, that the power and utility of the reſinous and fetid gums, conſiſts in their action "as * conductors or re-"tainers of ſomething very ſimilar to electric mat-"ter in the ſyſtem:" but although I am induced to favour the ſame opinion, and could ſupport it, by what I conceive to be, very cogent arguments, the conſideration of this ſubject would neceſſarily extend my obſervations to too great a length: I ſhall, therefore, content myſelf, *at preſent*, with ſtating my opinion, that the purely convulſive Aſthma is never found to ariſe unleſs the real cauſe of humoral Aſthma prevail, in a greater or leſs degree.

I have now completed the taſk which I had propoſed to myſelf,—namely,

I. To offer a few remarks on ſome paſſages in your "Practical Enquiry on Diſordered Reſpi-"ration."

* Bache on Electricity.

II. To

II. To point out the exiſtence of *acrid matter* in the *blood veſſels*, in caſes of Aſthma.

III. To ſhew the manner in which the ſerous effuſion into the pulmonary veſicles takes place.

IV. To explain the *nature* of the alteration which is produced in the *properties* of the blood.

V. To trace the offending matter to its *origin*, and to diſcover the *real cauſe* of Aſthma.

VI. To ſuggeſt the principles on which its cure may be ſucceſsfully undertaken.

I am aware that all Theory, though ever ſo well ſupported by the ſolid proofs of ſound reaſoning and accurate experiment, is liable to objections, which envious captiouſneſs in ſome, and a laudable jealouſy of innovation in others, are extremely ready to advance.

It may be the fate of my opinions to meet with ſuch objections; but as the attainment of TRUTH is the great object of my views, I ſhall be happy to ſee the arguments which can be brought forward, in oppoſition to what I have advanced, *fairly* ſtated; and will endeavour to meet them with candour.

If they ſhall be found to ſubdue the force of what I am able to adduce, in ſupport of theſe opinions,

opinions, I ſhall kiſs the rod, with ready and reſpectful ſubmiſſion, and abandon, as it will become me, any errors which I may have propagated, to juſt correction: for if the improvement of medicine, and conſequently the intereſts of ſociety, can be advanced, by the ſubverſion of my Theory, I ſhall relinquiſh it without regret, and be the firſt perſon to rejoice at the deſtruction of the fabric which I have erected; if its being razed to the ground, may only make room for a more perfect ſtructure, though *not one* of the materials which I have collected, ſhould be uſed in the building of it.

The importance of the ſubject which I have endeavoured to elucidate, will, I truſt, recommend my remarks to the ſerious conſideration of the faculty: and as you have excellently obſerved, that "good humour never fails to return with the freedom of reſpiration", I hope, at the ſame time when I congratulate you on your own cure, I may aſſure myſelf, that you will receive theſe pages with that generous candour which dignifies learning; and is the brighteſt ornament of ſcience.

I remain, Sir,

with all due reſpect,

your moſt obedient,

and moſt humble ſervant,

GEORGE LIPSCOMB.

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